THE LANCET.

VOL. I.] LONDON, SATURDAY, FEBRUARY 4. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

LECTURE XV.

NASAL CATARRH IN THE CAT, RABBIT, AND POULTRY.—THE VELUM PALATI, AND THE OS HYOIDES.

Distemper in the Cat.—There is another not very interesting or amiable, but very useful animal, which is a frequent victim to nasal catarrh and its sequelæ—the cat. The disease in the early stage is characterised, as in the dog, by defluxion from the nose and eyes, and frequent sneezing; but other portions of the membrane are sooner involved; cough succeeds; sore throat, inability to swallow, or even to masticate, swelling of the throat and head, drivelling from the mouth, rapid and extreme emaciation. The animal retreats to some corner, and there sits motionless and lost, and dwindles away and dies. If the poor beast is neglected, the disease is more fatal than in the dog, and far more infectious. If a distempered cat is in the house, there is not a dog in it that will escape.

The means of cure are the same, and are more efficacious. One or two emetics will, in the majority of cases, cut the matter short. The same medicines may, if necessary, be afterwards administered, but with a greater proportion of the mechanical vermifuge, for the disease is here invariably connected with the presence of worms.

In Rabbits.—Rabbits are subject to a similar disease, commonly known by the term *sniffles*. It more resembles glanders, inasmuch as it is longer and more decidedly confined to the membrane of the nose. The animal for many a week will exhibit no other indication of disease than a constant *sniffling*, and a profuse discharge of mucus and muco-purulent matter from the nose. At length other portions of the respiratory membrane, and soon after that, the constitution itself, become affected, and rapid loss of flesh ensues, and the animal pines away and dies tabid.

Like glanders and distemper, this is both generated and communicated. It is the frequent result of exposure to cold. A breeding doe, or a litter of young rabbits placed in a cold and damp situation, rarely cleaned and badly fed, will almost certainly have the sniffles; and if a rabbit labouring under this disease is admitted or suffered to remain among the others, three-fourths of them will be lost. In most of the diseases of rabbits, the abdominal viscera are implicated to a great extent. Here is a specimen of hydatids taken from the mesentery of a rabbit that died *sniffled*.

I have spoken of the sulphate of copper as a tonic, with its chief determination to the membrane of the nose. I have used it with considerable success in this complaint. It was given to some rabbits from the Zoological Farm, and the majority of them recovered. I afterwards sent some to a gentleman, whose stock of rare breeds of rabbits was rapidly diminishing from this pest, and the plague was presently and effectually staid. Emetics produced extreme distress, and the copper was therefore given at once in solution, in doses of from one to two grains morning and night. In this quantity it acted evidently as a tonic: the appetite returned, condition was quickly gained, but the discharge did not entirely cease in less than six or eight weeks.

In Poultry.—The feathered biped is even more subject to nasal catarrh than the quadruped. Many of our domestic poultry, confined in close and dirty places, and ill fed, have the *roup*, characterised at first by swelling around the eyes, and discharge from the nostrils, and drivelling from the mouth, at first limped, but soon becoming purulent and fœtid. The crop is particularly hard. This, however, only shows that the digestive organs sympathise with this affection of the respiratory ones, but cannot be considered as a diagnostic symptom of

roup. This complaint, although generated by bad management, is infectious in the highest degree.

Of the cure I cannot speak with confidence. Common salt is often given with good effect. It usually acts as an emetic. The dose for an ordinary-sized fowl would be half a teaspoonful of the saturated solution. Next comes cleanliness; the eyes and head generally should be frequently bathed with warm water. Warmth is indispensable—it is the secret of treating almost every morbid affection of the respiratory passages of our domestic animals, but not warmth in a close and ill-ventilated place. I have found occasional benefit from the pulvis antimonialis in doses of a grain, made into a pill with bread. The country people have a great many nostrums. At the head of them stand garlic and rue, beaten into a mass with butter; and they undoubtedly are serviceable, whatever be the modus operandi, whether they are diaphoretics or expectorants, or, rather, mild stimulants suited to a disease so apt to assume the typhoid form. Others give an apparently strange compound—rue and brick-dust, but not so absurd on second consideration as it would appear at first to be. There is the mildly stimulant effect of the rue, and the mechanical agency of the brick-dust, either in assisting the languid trituration of the food, or expelling the worms with which the intestines of these animals are often clogged.

Ducks are very subject to nasal catarrh, and frequently die almost suddenly. In *geese* it is recognised under the name of *gargle*, and is often successfully treated with garlic, beaten into a mass with butter. Even *pigeons* are not exempt from it. Whole dove-cotes are sometimes depopulated by it. There is the same defluxion from the nose and eyes and mouth, the same disinclination or inability to feed, the moping appearance, and gradual wasting. Garlic and rue are the common specifics here, but always in combination with warmth and cleanliness. When everything else has failed, it is said that little pills of the cordial horse-ball have roused the system, and enabled the animal to shake off the disease. Gentlemen, I make no apology for including these animals in our legitimate list of patients. If I understand our duty aright, it extends to every animal that we have domesticated, and that is a part of our property, and on which we have disease too often entailed. At all events they have life and feeling.

THE VELUM PALATI.

Now, Gentlemen, proceeding beyond the simple cavity of the nose, I must entreat your attention to a little anatomical detail; it shall be short, strictly confined to physiology or to practice; and the shorter because I am proud to inform you, that a young and talented and valued friend will soon commence the demonstration of every portion of every domesticated animal.

Attached to the crescent-shaped border of the palatine bone, you perceive a dense membranous curtain, a duplicature of membrane with interposed cellular and glandular and muscular substance. Its superior face is a continuation of the lining membrane of the nose; its inferior one that of the palate. It is called the *velum palati*, the veil of the palate: it hangs as a curtain or veil at the back of the palate.

It is attached to the whole of the crescentic border of the palatine bone, and also to the sides of the *ductus communis narium*, and also, as you will perceive, to the side even of the dorsum of the tongue; and it extends backward as far as the larynx, and lies upon, and in contact with, the dorsum of the epiglottis. So that you plainly see that in its natural loose pendant state, it is a perfect veil, or curtain, interposed between the cavity of the nose and the mouth, and cutting off all communication.

The Horse breathes only through the Nose.—The portion which lies upon the epiglottis is, to a certain degree, loose; but so tied down by this long attachment, which you trace anteriorly, and also by muscles which I shall presently describe, that it will open but a little way, and in one direction only: it will open to let the pellet of food pass into the œsophagus, and it will close when any pressure is made upon it from behind, so that two singular facts necessarily follow. The first is, that the horse breathes through his nose alone, and this will account for the beautiful mechanism of muscle, and elastic cartilage and membrane, which we have already described, around the nostril, and which so beautifully expands it when, in rapid progression, a larger supply of air is required; and also the adaptation of the construction of the nostril to the different purposes for which the different breeds of horses are intended. If the nostrils are the only apertures through which the air can be admitted, they must be capacious, and easily expansible, and they are so to a degree seen in no other animal, and fully commensurate to the wants of the horse.

Vomits through the Nose.—It is also sufficiently evident that, in the act of vomiting, the contents of the stomach must be returned through the nostril, and not through the mouth. The horse is with great difficulty excited to vomit. Do not mistake here; the *velum palati* has nothing to do with this, but it depends on a singular construction of the cardiac orifice of the sto-

mach, which I shall hereafter have to describe. This membrane regulates the passage, or orifice, through which the contents are discharged. It presents a mechanical obstruction to its return through the mouth, and which is only partially removed when the effort to vomit is unusually convulsive, or the power of every muscle and every membrane is relaxed.

The interposed substance between these continuations of membrane I have stated to be cellular, muscular, and glandular; the cellular, in its separate form, exists only in a slight degree; the muscular portion deserves a moment's notice.

Muscles of the Velum Palati.—The muscles of the palate are two. The first is the *stylo palati*, the *tensor palati* in Mr. Percivall's anatomy of the horse, as published in the Veterinarian. It is a beautiful muscle; trace it from the styloid process of the temporal bone, closely united with, and seeming to form a common tendon with the stylo-pharyngeus, running along by the guttural pouch of the Eustacian tube, and, in some not well-defined way, influencing that auditory passage, and then passing through a curiously-formed cartilaginous loop at the extremity of the pterigoid process of the palatine bone, as perfect a pulley as ever was constructed; it here turns round, and is inserted into the posterior lateral part of the velum, or rather ends in an aponeurotic extension over it. This muscle tightens the palate, and so lifts the floating fringe of it.

The other muscle is the *circumplexus*, arising from the border of the palatine bones, and diffused over the whole of the velum to tighten and to elevate it. The office of both is united; the first tightens the velum, and enables it to afford resistance to substances pressing upon it, and in doing so, in some measure elevates it; the second, while it likewise tightens the palate, elevates the border still higher.

The velum palati in the ox does not descend so low, and there is sufficient room left between it and the larynx for the purpose of breathing, and for the regurgitation of the food; and in conformity with this you will see that the tensor muscle is not so large, but the circumplexus is more developed and stronger. There is not so much resistance wanted, but greater freedom of action. The ox breathes partly through the mouth.

In the dog, who breathes almost entirely through the mouth, the *velum palati* is still smaller, the *tensor* weak, and the *circumplexus* powerful. When coryza runs on to catarrh, and the membrane of the pharynx sympathises with, or rather partakes of the inflammation, the velum palati becomes inflamed and thickened; it will not then act as a perfect veil, or curtain; it will not adapt itself to every part of the opening, and there is a communication between the passages of the mouth and nose, and thus we frequently have the defluxion from the nose, tinged by the colour of the food or particles of food mingling with it. We do not like to see this, because it proves to us the extent and the violence of the inflammation.

Pharynx.—The pharynx, at which we are now arrived, is a membranous, muscular, funnel-shaped bag, extending from the root of the tongue to the larynx and œsophagus; wider anteriorly at the back of the velum palati, and narrowing backwards until it terminates in the œsophagus. It is a powerful agent, by means of the muscles with which it is lined, in conveying the food from the mouth to the upper part of the gullet. This being its office, it belongs to the digestive system, and will be considered when we treat of that system. There is a bone, however, which constitutes a portion of its wall on either side, and common to it, to the tongue, and to the larynx, which must be now considered.

THE OS HYOIDES.

The Os Hyoides of the Horse.—The os hyoides is so named from its resemblance, in the human being, to the Greek letter Upsilon. In our patients the similarity can scarcely be traced. In the horse it has much more likeness to a spur, and the semicircular body, and the neck of the spur are perfect. In the young animal this bone is divided into seven portions; in the adult animal it is clearly divisible into five. The *body* embraces, but not closely, the thyroid cartilage of the larynx, for you see that there is a cartilaginous, ligamentous substance intervening. This bone gives protection and support to the thyroid cartilage, and also attachments to several important muscles, as the *hyo glossus longus*, to draw the tongue within the mouth, and the *brevis* having nearly the same office; and the *hyo-pharyngeus* to dilate the pharynx, and the *anterior constrictor pharyngeus* to contract the pharynx, and some other muscles. The principal muscles of the body of the os hyoides, are the *hyo-thyroideus*, from the inferior border to a tubercle on the postero-inferior side of the thyroid cartilage, to elevate it and the larynx, or to depress the os hyoides; and the *hyo-epiglottideus* from the hollow opposite to the neck, to the back of the epiglottis, drawing it from and enlarging the opening into the trachea. The spur or neck projects anteriorly from two to three inches in length, and pierces deep into the root of the tongue; its sides are roughened to give attachment to the *genio-hyo-glossi* muscles at the inferior part of the tongue, draw-

ing down the tongue within the mouth, and limiting its action.

Attached by a perfect capsular ligament to the extremities of the body of the os hyoides, and rising from it somewhat obliquely, are two bones, termed the *shorter cornua*, or horns; they give a certain degree of motion to the body of the bone, and also attachment to some of the muscles which are lost in, and constitute the body of, the tongue. There is also a muscle, *the transversalis*, running from the anterior tubercle of one of these cornua to the other, limiting the motion yet strengthening the action of these bones; and also a more powerful muscle, the *hyoideus parvus*, mentioned by Percivall, in his "Muscular System," but not afterwards described by him, from the shorter cornu to the longer, occupying the whole of the triangular space between them, and confining or bringing them close together.

United to the shorter cornua, and likewise by a proper capsular ligament, and constituting perfect joints, are the *longer cornua*, extending posteriorly and horizontally along the side of the pharynx, until they reach the petrous portion of the temporal bone. A little before this they begin to widen, and terminate in a kind of foot, the superior part or toe of which has a cartilaginous union with the temporal bone, and to the heel or inferior portion of which are attached two important muscles. The first is the *sterno-thyro-hyoideus*, and which arises even so low as the point of the sternum, and climbing up the neck above the *sterno maxillaris*, about the middle of the neck divides into three distinct bodies, yet united together by a slender tendon—the central one going to the lower border of the thyroid cartilage, and the lateral ones to the heels of the longer cornua of the os hyoides. Its use is to depress the os hyoides and larynx by drawing them downward and backward.

The other muscle attached to this process is the *hyoideus magnus* (the *hyoideus* of Mr. Percivall). It arises from the heel of the cornu, and, as it proceeds along the side of the bone anteriorly, its belly is divided, and forms a loop, through which passes one of the tendons of the *digastricus*. It has two insertions into the body of the os hyoides. This likewise draws the bone backward, or causes it to contract a little upon itself, and so helps to dilate the glottis.

These muscles deserve a fuller demonstration than I have time to give them, and you will ere long have the opportunity of attending on that demonstration. I have been compelled to mention them, in order that you may better understand the connexion and function of these important parts.

The Os Hyoides of the Ox.—You will at once perceive a considerable dissimilarity of structure between this bone in the horse and the ox. The spur is almost vanished—nothing remains but a little tubercle, to which indeed the same muscles are attached, but not with equal power. The shorter cornu on each side is multiplied into two, and the larger cornu terminates in a bifurcation of bone; and one of these bifurcations is not merely united to the temporal by cartilaginous adhesion, but a curious socket is formed between the mastoid process of the temporal bone, and a plate of bone let down for the purpose, in which the extremity of the cornu swelled out into a head or tubercle is received, and plays loosely yet securely. It is evident then that the tongue, freed from its attachment to a bone that runs deep into its root, has a more extensive action, and that this freedom of action is increased by the additional joints formed by the second shorter cornua.

I am not aware of any animal whose tongue requires so little extent and free dom of motion as the horse. He is not signed to graze closely, and his ton seldom or never employed in gath food. He, beside, is doomed to and often cruelly restrained by tongue of considerable or of l would be an inconvenience to the unequal pressure of the roll from one side to the other, riated or lacerated by the teeth. down by the spur or appendi bone, it forms a useful pad on wh may rest, and on which it may wo jaw which, even now, is too often by the bit, would be exposed to ye frequent and severer injury. Therefore tongue of the horse is thus confined. know but one other animal, the hare, in the hyoid bone of which this spur is found.

The ox has need of a far more flexible tongue. He bites much more closely to the ground, and, being destitute of teeth in the upper jaw, their place being supplied by a mere pad, he needs the aid of his tongue to curl round the grass to bring it into the mouth, and to help to hold it there until it is partly bitten and partly torn asunder. If you will observe a horse and a cow while grazing, you see not, or scarcely see the tongue of the one, while the tongue of the other is protruded at every bite, and is wrapped round the grass in the manner to which I have just alluded.

Grazing thus closely to the ground, the ox is subject to an annoyance from which the horse is in a manner free. He is continually getting dirt and filth and thorns and insects about his muzzle thus brought into contact with the ground, and he must have some means of ridding himself of the

nuisance. He has it in the length and freedom of motion of his tongue. You will rarely see the tongue of the horse protruded from his mouth, except he has acquired the trick of licking his manger; but the ox is frequently employed in cleaning his muzzle, and his flexible tongue will reach even to his nostrils. When two horses are expressing their mutual friendship, it is by nibbling each other; oxen will stand licking each other for an hour at a time.

It was to give freedom of the tongue to the one, and to take it away from the other, that the ox had no projecting spur, and two perfect joints on either side, while the tongue of the horse was tied down by this inflexible bone piercing its root, and its freedom still more impaired by one joint only being given to the hyoid bone.

The Os Hyoides in the Dog.—Here will you perceive that we have not only a deficiency of spur, but also of tubercle. There is even [illegible]pression in the centre of the body of the [illegible]d there are two shorter cornua on [illegible]. The tongue of this animal re[illegible]s freedom as, or even greater [illegible] ox, for if he has not to [illegible]od together, he has to lap [illegible]d.

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With regard to the difference of muscular structure I will not detain you. You will perceive that the *hyoideus magnus* in the ox has no pulley for the insertion of the digastric, and that the stylo-hyoideus is larger and digastric, while in the hog it is, like the longer cornu, of comparative insignificance. The genio-hyoideus is much more developed in the ox than in the horse.

LECTURE XVI.

ON STRANGLES.

I know not where better to treat of a disease, from an attack of which, of greater or less intensity, the horse seldom escapes, —I mean inflammation of the cellular substance immediately beneath the anterior portion of the hyoid bone, and known by the term strangles. It is peculiar to young horses, and generally attacks them when they are between three and four years old.

Symptoms.—The horse is dull, feverish, off his appetite, does not thrive, his coat stares. Cough comes on—it is treated as common cough, but it gets worse; there are violent, almost suffocating fits of wheezing and coughing—slavering from the mouth —swelling of the tongue—discharge from both nostrils, usually a yellowish colour, and soon of a purulent character, yet rarely accompanied by any offensive smell. The fever increases—the pulse indicates extreme irritability—the cough becomes more aggravated—the horse heaves violently at the flanks—and this continues three or four days, or perhaps a week; and if the practitioner has neglected to examine the age of the horse, and has not recollected the disease to which he is then so subject, but has treated the case as one of aggravated cough and fever, and has bled copiously, and has been pouring in his sedative medicines, he may linger on another week, and not do well at last.

At length some swelling appears about the throat or head; at first perhaps in the region of the parotids; but at length the enlargement is decidedly in the centre of the channel between the branches of the lower jaw. The disease now assumes its true character, *it is phlegmonous inflammation of the subcutaneous cellular substance between the jaw.* It is called *strangles*, from the violent cough before the appearance of the tumour, and the pressure of the tumour before it suppurates threatening to strangle the horse.

Derivation of Name.—Mr. Castley, in a late number of the Veterinarian, quotes from Gervase Markehan, the probable derivation of the name. "It is a great and hard swelling beneath a horse's nether chaps, upon the root of his tongue, and about his throat; which swelling, if not prevented, will stop the horse's windpipe, and so strangle and choke him, and from which effect, and none other, the name of this disease took its derivation." In some parts of Great Britain it is called *strangullion*. The inhabitants of the north of Italy recognise it by the name of *strangolina*, and in the south of France it is termed *étranguillon*. The prevailing symptoms are

ing down the tongue within the mouth, and limiting its action.

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The ox has need of a far more flexible tongue. He bites much more closely to the ground, and, being destitute of teeth in the upper jaw, their place being supplied by a mere pad, he needs the aid of his tongue to curl round the grass to bring it into the mouth, and to help to hold it there until it is partly bitten and partly torn asunder. If you will observe a horse and a cow while grazing, you see not, or scarcely see the tongue of the one, while the tongue of the other is protruded at every bite, and is wrapped round the grass in the manner to which I have just alluded.

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Of the Swine.—We have here also the hollow instead of the spur or the tubercle, for the swine nuzzling in search of his filthy food requires considerable use of the tongue. You will observe the singular form of the cornua in this animal, the strength of the first shorter one, the smaller development of the central one, and the slimness and insignificance of the longer one, compared with that of any other animal that we have examined. You will observe likewise that the interposed ligament between the body of the bone and the thyroid cartilage (the hyothyroid ligament) is strangely diminished, the bone seems almost closely to embrace the cartilage. Of the physiological reason of these differences of structure, I confess that I am unable to speak. Veterinarians have not yet deemed them worthy of attention. The time however is at hand, when our duty will be better understood and practised.

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Derivation of Name.—Mr. Castley, in a late number of the Veterinarian, quotes from Gervase Markehan, the probable derivation of the name. "It is a great and hard swelling beneath a horse's nether chaps, upon the root of his tongue, and about his throat; which swelling, if not prevented, will stop the horse's windpipe, and so strangle and choke him, and from which effect, and none other, the name of this disease took its derivation." In some parts of Great Britain it is called *strangullion*. The inhabitants of the north of Italy recognise it by the name of *strangolina*, and in the south of France it is termed *étranguillon*. The prevailing symptoms are

much the same wherever the disease is known.

Certain Countries exempt.—Strangles is unknown in certain countries. It has not yet found its way to the south of Italy, nor to Spain, and neither to Arabia nor to Africa, and northwards it has not reached to Russia or Norway. We cannot always give the reason of it, but the fact is plain enough, that certain diseases of the human being and the brute are peculiar to certain countries.

Epidemic.—The subject of atmospheric agency is yet involved in a great deal of mystery, but we know, with reference to this disease, even in our own country, that there are certain seasons when a case of strangles rarely occurs, and a few months afterwards there is scarcely an establishment or a farm, without the majority of the young horses exhibiting evident symptoms of this disease. Whether it is simple epidemic, or both epidemic and contagious, is doubtful.

Contagion.—Some of our best writers speak of the advocates of the contagiousness of strangles in the sarcastic tone of contempt. For my own part I am perfectly assured that it is epidemic, but there are cases which I cannot explain, except on the supposition of its being contagious too; and I could never bring myself to think or to speak contemptuously of the opinion of almost every old writer, and of almost every country practitioner of the present day. Colts at grass have strangles much more mildly than the stabled horse; indeed the tumour and the discharge are sometimes so small, that they elude observation, if the horse is not taken up and closely examined. When speaking of glanders, I have told you that the careful observer can scarcely confound strangles with glanders. The size and situation of the tumour will be an infallible guide.

Treatment.—I will suppose that you are called to a case of strangles. There is no doubt about the nature of the disease. Then take it as a principle, that this phlegmonous inflammation must run its natural course—the tumour must break, and its contents must be discharged. There is something lurking in the constitution which is a source of annoyance, and must be got rid of. This tumour is a spontaneous effort of nature, and once completed, there is seldom need for repetition of it, at least for many years. Some old horses have had strangles a second time. It has been said that others have not had it at all, or they have had it so mildly that it could scarcely be distinguished: and, like distemper in the dog, a disposition to be severely affected by it seems to run in the breed. If it is repelled, the horse continues poorly and feverish, and other swellings form about the throat or in other parts, and proceed to suppuration.

Nature of it.—Mr. Castley has published an excellent paper on this disease, and I think that the veterinary profession is much indebted to him for it. He considers that the tumour and abscess of strangles is one of the methods by which nature gets rid of that which annoys and oppresses her. He tells us, that when a farmer sees a young horse looking sickly and delicate and thriftless, he says that he is breeding the strangles, and that he will not be better until he has got over that complaint.

This is the case with many eruptive and other diseases of the human being. The constitution struggles for a while to bring forward the disease, and until it is brought forward the patient is not himself. You will find this to be perfectly true with regard to strangles before you have seen a great deal of practice. You will find the colt struggling for months together to throw off the incubus by which it is oppressed, and when that is done, thriving with a rapidity before unknown.

Cases.—Mr. Castley gives us some cases in illustration of this. A bay gelding, four years old, had cough, but unaccompanied by any swelling of the glands or sore throat. This continued during a whole month in spite of medical treatment; at length a tumour began to form under the jaw, and a small abscess ensued. Another tumour appeared on the cheek, which was opened; and then there was a collection of matter among the muscles of the fore-arm, and, lastly, over the hip-joint. These were properly attended to and healed, and the horse immediately began to improve. This was evidently a case of strangles at the beginning, for it was preceded for more than a month by a troublesome cough, and this disposition to abscesses every-where, arose from the slowness with which the natural tumour of strangles proceeded to suppuration, and the imperfect manner in which it did suppurate. He relates another case in which, after apparently recovering from strangles, a mare of three years old sickened and died, and a large tumour was found in the mesocolon, filled with thin purulent matter. He considered it, and rightly I think, as connected with strangles, or one of its sequelæ.

Mr. Tombs mentions a case of strangles, complicated with large tumours in the abdomen, and particularly with one on the kidney.

I was sorry to read, while preparing myself for this lecture, in our very best veterinary work, "that the common notion, that unless an abscess formed and was evacuated, the animal did not derive that

constitutional benefit which nature intended him, is one of those vulgar chimeras, that seems not upon any rational grounds to be deserving of more attention in practice than is requisite to appease those who are determined to follow the humoral or farrier's pathology." I honestly confess that I am one of those who are disposed to adopt this farrier's pathology, for I cannot forget the numerous cases in which I have seen a puny weakly colt speedily thrive after strangles attended with proper discharge; and, on the other hand, the circumscribed tumours, more diffused swellings, want of condition, and almost worthlessness, which have followed strangles, either improperly treated, or not brought sufficiently to a head.

Treatment Resumed.—Well, then, our object is to promote speedy and effectual suppuration in the part. This is the grand object, whatever, in other points, may be our theory of the disease. Then, except there is considerable fever, there must not be any bleeding,—no lessened determination of blood to the tumified part,—no physic, and for nearly the same reason,—no cooling embrocation, for until the tumour is ripe we must encourage, and not diminish the local inflammation,—no poultices, for they are comparatively ineffectual through the thick skin of the horse, to say nothing of the difficulty of confining them there, and the coldness of the part, and arrestment of the healthy inflammation when the poultice is displaced, or the part exposed to the cold,—no fomentations, for you cannot rub the part dry after it has been fomented, and the cooling, delaying process of evaporation will commence. If you will use a fomentation perhaps you will adopt Mr. Parkinson's, consisting of water in which elder-leaves and white-lily-roots and groundsell and house-leek have been boiled, and then a quantum suff. of chamberlye and cow-dung added. Since I have got into the habit of giving quotation upon quotation, I will refer you to old Dr. Bracken, whose book is on the whole a treasure. He recommends "a poultice of mallows and marsh-mallows, and other such potent applications, which is a most incomparable cataplasm; for when nature is, as it were, despouding, and cannot throw off the load of matter that causes the distemper or swelling, this by its genial warmth comforts the stretched fibrillæ or animal threads that are distended beyond their natural tone, and also greatly contributes to thin the skin by its mollifying and softening qualities." You will not perhaps do exactly this, but you must go more earnestly to work.

Blistering recommended.—You must stimulate the part, not as Mr. Parkinson again writes, by holding a lighted candle to the part to make it break the sooner; and, as another writer recommends, to hold it there until you have fairly burnt into the tumour; but by applying a good sharp blister. You will save many a day, perhaps some weeks of indisposition, and much weakness and emaciation, the necessary consequence of a long struggle between the constitution and the disease. Prompt blistering forms the grand secret of the treatment of strangles.

When you have brought the tumour to a state of suppuration, do not open it too soon lest you should prematurely arrest the process. On the other hand do not leave the tumour to break lest you have a ragged ulcer that will not heal. As soon as the swelling is fairly softened, lance it, and lance it freely, or the orifice may close before all the matter is discharged. Nothing should be done to open the tumour before the process of suppuration is matured, except the enormous bulk of the tumour, and its pressing on the trachea, and threatening suffocation. The swelling has in some cases so enormously increased, and the process of suppuration has been so slow, that it has been necessary to resort to tracheotomy in order to save the animal.

Treatment after the opening of the Tumour.—You will now give a slight aperient—probably a drachm of aloes in the common fever ball, for a few successive mornings until the dung is loosened. You will keep the horse on green meat or mashes, frequently foment the wound, or apply a poultice over it to keep up the discharge as long as possible. That beginning to cease, you will inject a little tincture of aloes into the orifice morning and night, and the wound will heal quite as rapidly as you could wish. The wound being healed, a mild dose of physic will be highly proper; and in order to carry off any humour that may remain, a portion of green meat should be continued during a few weeks, the horse gradually returning to his proper quantity of corn.

In general cases, the restoration to health and strength may be left to the salutary power of nature, but occasionally strangles leaves a considerable degree of weakness. Tonics are then indicated, such as the gentian and the ginger; and, in cases of great debility, the sulphate of iron. I need not tell you that just in proportion to the degree of weakness in which the animal is left will be the danger of attempting too rapidly to restore the power and condition of the horse. Corn must be given very cautiously, and tonics administered with much circumspection—the vegetable tonics first, and afterwards the mineral one. If the weather is tolerably warm, there cannot be better practice than turning out during the day, and taking up at night. The cough of strangles will long remain, after the original exciting

cause has been removed, and a little imprudence will make it degenerate into chronic cough, or broken wind, or, perchance, some dangerous inflammatory affection of the chest.

Bastard Strangles.—The disease so termed I am unable fully to describe. It assumes as many forms as the distemper of the dog. It will be recognised by some enlargement between the branches of the jaw, in the same situation as the tumour of strangles, and accompanied by more or less fever, purulent discharge from the nose, and that sometimes profuse: but the swelling does not proceed to suppuration. It teazes for many a week, and then begins to lessen, and at length leaves behind it a very small hard tumour, which is perceptible for months or years, but is not tender and does not seem in any way to annoy the horse.

All this is evidently an ineffectual attempt of the constitution to bring forward and ripen the tumour of strangles, and the mode of assistance most likely to be successful is the application of local stimuli in the manner that I have described.

Vives.—The term vives is used with much ambiguity by veterinary writers. It sometimes refers to those little knots or kernels that are left under the jaw when the tumour of strangles has subsided. They are distinguished from glanders, by their being larger than the kernels of that disease, and not adherent to the jaw, but situated towards the middle of the channel. The better way is to let them quite alone. If any thing is applied, it should be a very diluted turpentine tincture of cantharides. A slight irritation of the skin kept up for some time, may rouse the absorbents to their duty, and cause the little tumours to disappear.

Other writers, by the term *vives*, understand inflammation and enlargement of the parotid gland, but this is a salivary gland, and belongs to the system of digestion.

STRANGLES IN THE OX.

This disease is also recognised by the term *strangullion* in many districts. It is accompanied by fever, loss of appetite, impairment of strength, cough, and, after a while, swelling about the throat, seldom a very prominent one, but of a more diffuse kind, very hard and tender. In two or three days it points, but rather obscurely, and may then be opened; it rarely breaks, for the skin of the ox is thick and tough; but the long presence of the matter irritates the animal, and produces serious illness, and sometimes extensive sinuses. The emaciation of the animal, when the tumour continues long, or is improperly treated, is incredible.

The treatment is the same as in the horse; blister, and blister actively. Bleeding is somewhat oftener allowed, but should not be resorted to unless very considerable fever prevail. The discharge from the matter being longer retained, owing to the indistinct pointing, or fluctuation, is oftener fœtid and bloody than in the horse. The chloride of lime will remove the fœtor, and the tincture of aloes will heal the wound. A physic drink (one pound of Epsom salts with half an ounce of ginger) will be useful, and the same tonics may be administered if the debility be very great.

SWELLINGS UNDER THE THROAT IN THE DOG.

A phlegmonous tumour, precisely in the same situation, and accompanied by as much constitutional disturbance, with the exception of there being little or no cough, often appears in the dog. Comparing the size of the animals, these tumours are much larger in the dog than in either the horse or the ox, but they run more up the face, and do not press so much upon the windpipe, nor do I recollect any apparent danger of suffocation from them; but the whole head has been enlarged to a frightful degree, and the eyes have been completely closed. More than a quart of fluid has sometimes escaped from a middle-sized dog at the first puncture of the tumour.

The mode of treatment is the same in order to stimulate the part, to expedite the suppuration of the tumour, and to lance it freely and deep, as soon as matter is evidently formed. The wound may be dressed with tincture of aloes, but a thick bandage must be placed round the neck to prevent the dog from scratching the part, and causing dreadful laceration.

Cystic Tumours.—These tumours in the throat of the dog are not always of a phlegmonous character; they are cysts, sometimes rapidly formed, and of considerable size, and filled with a serous or gelatinous fluid. Lancing will here be of no service, for the orifice will heal and the cyst will fill again. A seton must be passed through the tumour, and worn until the cyst is obliterated by the adhesion of its sides.

Veterinary Hospital at Lyons.—An account of the animals treated at this school between August 1, 1830, and August 1, 1831, has been published, and we extract from Mr. Youatt's excellent periodical *The Veterinarian* the following summary:—There have been cured or discharged from the hospital, 290 horses, 13 asses, 14 mules, 2 cows, 229 dogs, 14 cats, 2 goats, and 1 sheep. Of 62 horses which died or were killed, 13 died of staggers, 4 of farcy, and 13 died or were killed glandered. Of 74 dogs which died 31 were rabid.

LONDON HOSPITAL.

CLINICAL LECTURE

DELIVERED BY

DR. BILLING,

PHYSICIAN TO THE HOSPITAL,

Dec. 17, 1831.

NEW FEVER.

SYNOCHA PETECHIALIS.

I WISH, Gentlemen, to-day to call your attention to a subject of considerable interest,—the *new fever*; not that I mean to assert that there is anything new under the sun, or that I have made any discovery, but I feel satisfied that the peculiarity of the present epidemic has not been pointed out to you, nor will you find it in Cullen's Nosology, at least not recognisable from any of his definitions; but so far as I can judge from observation and comparison, it was described by Sydenham 150 years ago, and I shall now lay before you the result of my investigation. Last year, before it came to my turn to take up the clinical business in May, I had heard much of the fever* in the hospital, and had sent in a few cases which occurred among the out-patients, and several absurd reports were circulated out of doors, in consequence of the timidity and gossip of the nurses and others; I was told at the west end of the town that we had got the plague, or a plaguey typhus, at least, in the London Hospital, and that there was a black flag hoisted, and I had almost as many questions put to me then about the London Hospital fever, as lately about the cholera. Upon inquiry I found, in fact, that there had been a rather greater number than usual of febrile cases, and that, most injudiciously, they had been all crammed into one ward, so that by concentrating the effluvia, it was rendered noxious in the proportion of four or five to one. In consequence of the new building, one half of the ward (Harrison's) had been dismantled, and the two open door-ways of communication between the two halves, which ordinarily contribute so much to ventilation, had been boarded up. Mr. Williams, whom I had taken with me to show me the cases in question, pointed out four fevers in one corner of Harrison's; when I remarked to him the danger of this arrangement, and how close and unusually foul the air of the ward was, he pointed to the boards and said, that he had had a plank removed from each place for ventilation, but that the chaplain had ordered them immediately to be nailed up again, that he was afraid to visit the sick if labouring under fever, and therefore had turned Harrison's ward into a kind of lazaretto; hence arose the reports and alarm. One nurse, it was said, died of fever, another of fright; any patient who could not help himself, ran the risk of being without drink, whether fevered or not, and I saw that one of the under nurses, whom I ordered to give a drink to one of the patients, was much alarmed at the supposed risk she ran, but did not disobey; nay, the ward got such a bad name, that a month after, when I desired one of my convalescent patients in George's ward, to be removed into Harrison's, he begged to be allowed to leave the hospital instantly at any inconvenience in preference.

In a few days after this visit, I resumed duty in the wards, and, in the first place, put every fever which came to me, into George's ward, until the stock in the other was reduced to its fair proportion, and, in the next place, took care that no fever should be in a corner, nor within five or six beds of another. No person took fever from any of my cases, and the alarm died away.

I knew from experience the propriety of this arrangement; in one hospital, which I attended for three years, we had always several cases of fever, and for two years no communication of it; in the third, however, the directors thought fit to appropriate one ward as a fever ward, when, in consequence of the concentration of the effluvia, the fever was communicated to some of the attendants, and a patient, convalescent from rheumatism, who came into the ward, was attacked with fever.

I had heard the cases had been typhus, but I found those which came to me had not the character, for though they had petechia, the pulse was not weak and small, nor the delirium low and muttering, but rather talkative, though not phrenitic, and some of the patients inclined to get out of bed, instead of lying prostrate as in typhus. There have been occasional cases of typhus also, and I have had opportunity of proving to the class that fevers are curable without either wine or mercury. Wine you know I do not prescribe in typhus, except on very extraordinary occasions, and late in the disease; even if I should fail to convince you of the impropriety of using wine early in true typhus, let me teach you to distinguish synocha when so masked as to appear typhus. I say synocha, as I do not use the term synochus; yet though I do not use the term synochus, I recommend you to study the chapter so designated, as well as the whole of Dr. Southwood Smith's book upon fevers, which

* During part of the year, Dr. Billing exchanges duty with his assistant, and prescribes for the out-patients.—*Rep.*

is, in my opinion, the best and most practical work on the subject; and observe that where he recommends stimulants, it is not until the disease has been conquered by remedies, or has come to that state of approaching collapse which the ancients designated by the term crisis, and the Italian school calls a change of diathesis.

Mercury is used indiscriminately in fever by some practitioners, and, provided antiphlogistic treatment be not neglected, does no harm; and calomel as a purgative, improving the secretions and excretions, does good. But I have shown you, and will still, that antiphlogistic treatment, without salivation, is sufficient, and it is no joke to loosen a man's teeth unnecessarily. But there is a still stronger reason. If the antiphlogistic means are not sufficient, the business will be over before there is time for mercury to affect the system, so that if you depend on the mercury alone you will lose your patient; and, if the antiphlogistic treatment in the early stage, and quinine, with stimulants when required, in the after stages, can effect a cure, why put the patient to the inconvenience, if no worse, of salivation? My opinion is, that some good practitioners use mercury unnecessarily, and that some who fear to employ evacuations, deceive themselves in their reliance upon mercury without them. To confirm this to you, though I employ calomel so frequently as a purgative in general, yet to avoid any equivoque, I prescribe other aperients instead of it in the cases of fever, that you may have no suspicion of mercurial influence. It is in chronic or lingering affections, that we find affecting the mouth by mercury of use, and we then have time for its employment.

Cullen calls the eruptive fever of smallpox, of measles, and of scarlet fever, synocha, though each of them occasionally becomes typhoid. Why then use the term synochus? It may be said that it is a mixed fever—that is my objection; mixture is jumble, and jumble confusion, and though we cannot always escape it, let us endeavour to do so; and in the hospital at this present time, during this epidemic, you have much better opportunity than in private practice of seeing how patients (from want of remedies, or injudicous administration of stimulants by their friends, and fatigue in being brought here, often on foot, and exposed to the inclemency of the weather) appear typhoid when they present themselves, though labouring under this epidemic, which is synocha—that is, inflammatory fever—that is, complicated fever; for I acknowledge no simple fever but typhus, or, if idiopathic, complicated with inflammation of viscera or meninges, causing that hardness or strength of pulse, which does not exist in typhus; which typhus is disease of the cineritious part of the nervous system, of the brain and spinal cord, which cineritious part is the *instrument* of the soul, without which it can do nothing in this world.

You have violent synocha, with phrenitis, whether that be idiopathic or from a blow on the head, which free bleeding will cut short; you see the synocha of the exanthemata often stop when the eruption is perfected. Who can cut short typhus in this manner, or has seen genuine typhus spontaneously and suddenly stop? The most violent synocha then, I say, may have less of the essence of fever than a mild typhus; there is plenty of pyrexia, but pyrexia is only a part of fever, disturbance of functions; the most violent disturbance of cerebral functions in synocha, is but pyrexia, and may pass off as quickly as intoxication from fermented liquors. Not so typhomenia, which is analogous to concussion, which requires a long time to recover; but you may have concussion and meningitis from the same accident or accidents, and then you have a mixed case or cases, showing all gradations, from much stupor with little inflammatory fever, to little stupor with much inflammatory fever, analogous to the gradations of typhus with complications, to synocha. Now, the cases of synocha are liable to put on a deceptive appearance of typhus, from a variety of circumstances, and occasionally from oppression of the lungs, when that is the complication, as is the case in the present epidemic.

I shall now give you Sydenham's account of the mode in which this kind of fever was first observed by him, and which made him denominate it the *New Fever*, divesting the account of his hypothesis and preserving the valuable practical parts.

After agues had been very prevalent for some years, so as to be considered epidemic, there occurred a very severe frost in the year 1683: the following winter was not much milder; and, on the breaking up of the frost, in the month of February, arose that fever, which he, with all his experience, considered as a new species, or at least as undescribed. At first, he says, he considered it a *peripneumony notha, or bastard peripneumony, only milder than usual in its symptoms, but that he afterwards found he was mistaken. He also institutes a comparison between the new fever and measles; he says, petechiæ often break out, and sometimes eruptions which are called miliary, not much unlike measles, except that they are more red, and do not leave the branny scales of measles.

The symptoms he gives are these. First

* Vide page 503 of THE LANCET.

alternations of flushes and chills, pains of head and limbs, pulse not differing much from the healthy standard, blood taken from a vein generally like that of pleuritic patients, for the most part cough, sometimes pain in the neck and fauces in the beginning of the complaint, but less than in quinsy; the fever of the continued character, though frequently with evening exacerbations, like a double tertian, or quotidian; a strong tendency to delirium, but the patient does not become frantic, as sometimes in variola, and other fevers, being rather tranquil, though incoherent in what he says; the tongue, when dry, is brownish in the middle with a white margin, but when moist, covered all over with a white furred pellicle. And when the pulse becomes irregular, with subsultus tendinum, death soon follows.

Now compare this statement with what has occurred to us. Agues have been notoriously, for several years, unusually prevalent in this neighbourhood. The winter before last we had much severe frost, last winter not so much, and, in the month of February, the cases of synocha petechialis, as we must denominate it to distinguish it from typhus, became so frequent as to attract observation; they were numerous until the middle of summer, when the epidemic remitted, but did not cease altogether, and the cases have now begun to show themselves in greater numbers again. It is of great consequence to distinguish those cases from typhus, because the petechiæ which occur, almost uniformly, have been too generally thought diagnostic of typhus, and this opinion might deter you from using the only means on which the safety of the patient depends, as you have seen demonstrated at the bedside, and you will no doubt have many more opportunities of judging of what I may here advance. After this general statement of the nature of the disease, let us consider the particular cases entered in the journal.

CASES.

F—— W——, æt. 20. Febris.

December 8. Complains of pain at the scrobiculus cordis, pains in the limbs, and weakness; has been ill since the 4th inst. Was attacked first with loss of appetite, weakness, giddiness, pain in the back and head, with stiffness, and soreness of the eyes. Has some tightness across the chest, and a very little cough, some rhonchus sonorus and sibilans on right side; is rather deaf; face flushed, eyes suffused, forehead hot; pulse 120, hard; papillæ of tongue white and inclined to dryness; much thirst; no appetite; bowels open freely since 3rd, when he took medicine; skin hot and dry; gives perfectly correct answers; respiration rather short; urine free, high-coloured; little sleep and some delirium at night; is dirty in his person.

Balm. tepid. ad 98° *statim; hirud.* xij, *serob. cordis; antim. tart.* gr. ¼ *ex aquæ,* ℥ss, 2is. *horis.*

This man came in earlier in the disease than we usually see in hospitals, as in general they arrive after delirium is established, and frequently with the petechiæ on them; in this case delirium was only commencing at night at first, as it does usually in febrile complaints.

The next day the pulse was 96, full, rather jerking, but not strong; the chest relieved, but more fever; the tongue dryer, and more delirium. *Lotio frigida capiti.*

For a few days the fever continued; the tongue dry, skin hot and dry, and he was more delirious, and inclined to get out of bed at night, for which he had the head shaved, some leeches applied to his temples, and as he complained of diarrhœa, the antimony was changed for inf. rosæ comp. It was not until the 13th inst., the ninth day of the disease, that there is a report of spots like freckles coming out over the skin; still some cough, and rhonchus sonorus gravis and sibilans, which, with the previous reports of the state of the chest and eyes, confirm Sydenham's analogy to measles, the difference being in date, and colour of the eruption, for the word freckles, here used by the clinical clerk, shows that they were remarked not to be of the purplish colour of measles, and not elevated. On the 16th, the spots, which had become redder, being still out, his pulse was 72, soft, the tongue moister, with a white coating, the skin warm and soft; he had slept well, was nearly free from delirium, and he had a strong desire for animal food, which is an unequivocal sign of convalescence in fever; no alteration was made except adding ℥j magnesiæ sulph. to each dose of his medicine, as the bowels were becoming sluggish. To day the eruption is fading; he still coughs; pulse 90, soft and full; he is now quite free from delirium, but he is still deaf, as occurs in several of these cases.

Febris.—J. G., ætat. 24 (extra case). Nov. 21. Admitted in a state of collapse from fatigue of coming to the hospital; ill clothed and squalid, incoherent, and when questioned complains only of pains in his limbs and body, and chilliness. Pulse weak: tongue furred; skin dry, but cool from exposure to atmosphere; he reported his bowels to be confined. He was admitted by Dr. Macbraire, who ordered him the only safe stimulant, the warm-bath, to restore the circulation to the surface.

Bal. tep. statim. Pulv. cal. c. jal. co. ℈j, *statim; antim. tart.* gr. ¼ *ex aquæ,* ℥ss, *ter die.*

According to the report of this case the synocha wore the mask of typhus.

22. Complains of the pains in his limbs and trunk, and also of his chest, and of troublesome short cough; rhonchus sonorus in different parts of the chest; pulse 120, full, and jerking; tongue has a thick white fur; bowels free; face flushed; headach; sensorium oppressed; thirst. I did not repeat the calomel for the reasons I have already stated, but as every symptom called for antiphlogistic treatment, I ordered him

Antim. tart. gr. ½; *magnesia sulph.*, ℥j; *ex aquæ*, ℥iss, *6tis horis. VS. ad* ℥xvj.

23. The report at 9 A.M., taken by Mr. Williams, was,—passed a bad night, was delirious, and could scarcely be kept in bed; tongue dry and brown; skin hot and dry; eyes suffused; pulse 120, and full; fæces passed involuntarily.

At 3 P.M. the tongue had become moister, and the skin moist and warm, and sprinkled with brownish petechiæ; the left tonsil was slightly tender on pressure externally; no redness in fauces. *Perstet.*

The state of the eyes, as well as the slight affection of the tonsil, as alluded to by Sydenham, forms a connecting link with scarlatina, as well as measles, though the disease is different from both; the spots are distinct, scattered; occasionally one as large as a split pea in diameter. The uniformity of the chest affection and petechiæ in all the cases, forms the character of an exanthematous disease; and there is generally an alleviation of symptoms, when they appear, unlike typhus; the eruptive fever is synocha; the pulse is, as Sydenham says, in many cases little different from natural, but always harder or stronger; and particularly during convalescence, it often happens that the pulse is even stronger than in health, unlike typhus. The delirium is not typhoid, for the patient answers you in general so directly, if not deaf, that you sometimes will scarcely detect his incoherence at first.

After this the fever continued, with skin hot and dry, for some days, but the symptoms gradually declined; he had leeches applied for tenderness of the abdomen, his bowels were regulated by saline or antimonial medicines, and he has become robust without any tonic medicine.

Febris.—J. T., ætat. 35 (extra case). Dec. 13. Five P.M. Admitted in a state of collapse, incoherent; pulse very feeble; cold extremities; says he has been six days ill. He was met in the street in this state, and conducted to the hospital. Mr. Williams ordered him

Balm. tepid. statim, et postea pulv. doneri, gr. x. *Vesicatorium nuchæ*, and next morning, as he had no motion,

Pulv. cal. c. jal. co., ℥j.

14 P.M. Makes no complaint; delirious; nurse says he had very little sleep in the night; pulse rather weak and slow, but irregular in frequency and strength, occasionally a full beat. Has difficulty in opening his mouth; tongue clammy; bowels not open yet; no urine; skin cool; subsultus tendinum; cough; rhonchus sonorus.

℞ *Ol. terebinth.*, ℥ij; *magnesia sulph.*, ℥j; *olei ricini*, ℥ss; *decoct. hordei*, ℥j, *ft. enema.*

Notwithstanding all the symptoms of debility, from the occasional single full throb of the pulse, I suspected the lurking synocha, and therefore endeavoured to relieve the brain and chest by an active enema, which would move the bowels with the least possible expenditure of strength. Observe, the period for bleeding was past, the collapse was permanent, for even after the warm-bath, when he was in bed, and warmth applied to the feet, no reaction, as it is called, took place; the skin continued cool; he presented the appearance of a person who is labouring under arachnitis, after the acute stage is past, when pain is gone, in which case the patient says he feels very well, the brain, at the same time, evidently being what is called muddled. The ol. terebinth was added for its antispasmodic property, which was indicated by the subsultus tendinum, and also on account of the deficiency of urine, which might be attributable to the blister.

The enema produced several motions, passed involuntarily, but as there was no urine passed even then, Mr. Williams, whose attention to these poor patients is unremitting, had the catheter introduced, and drew off a large quantity, showing you an example of what I have elsewhere inculcated, the necessity of not merely asking the attendants (who may be deceived by the liquid state of the fæces) whether urine is passed, but also of feeling the hypogastric region, in case the bladder should be distended. As the cough continued troublesome, and the fauces and tongue dry, he had the oily emulsion, with ℥v tinct. opii, occasionally.

Yesterday, 16th, the disease showed itself, the eyes became red, and the red petechiæ came out all over the face, trunk, and limbs. Tongue coated, white; pulse at rate of 84, irregular, still, but not weak; skin warm and natural; respiration free; less delirium, spoke more distinctly, and subsultus diminished; several involuntary motions, the urine free and not involuntary. One peculiarity of this complaint is, that the mucous membrane of the intes-

tines, as well as of the bronchia, is inclined to inflammation, as evinced by the tendency to diarrhœa, for though the patient at first does not evacuate the bowels (apparently from insensibility to the call), yet as soon as they are set going, either by medicine, or even an enema, they continue purging, and even involuntarily, from the same insensibility; but unless you do relieve the bowels, you will have the head much worse. Previous to the eruption, bleeding, antimony, and salines, are necessary; the antimony without the salines, if the bowels be free, whilst the skin is hot and dry. When the eruption comes out, there is generally an alleviation, and then quinine or decoct. cinchonæ is most useful in supporting the nervous system, for the disease lasts many days longer; if, however, after the eruption appears, you find the skin keep hot and dry, you may suspect that inflammation is going on in the bronchial or intestinal mucous membrane, as happens often in measles; if in the chest, you have rhonchus sibilans, &c., with the cough; if in the abdomen, tenderness on pressure, and griping, as the patient will now most likely begin to recover sensibility enough to make complaints, though still more or less delirious; according to the circumstances of the strength of the patient, you may then resort to leeches or blisters, and, as medicine, at the same time (or if the diarrhœa tends to become chronic), decoction of hæmatoxyli is more efficacious than catechu or other more astringent substances.

Another distinction, between this epidemic and typhus, is, the length of time the patient will go about, if not restrained by advice. This J. T—, who is by no means a pauper, was trying to pursue his business, when found and conducted to the hospital, not being able to tell where he lived. A gentleman, a patient of mine, went about trying to attend to business, and walked to a friend's house to dine the eighth day of the fever, and though he had complained of headach at first, yet on the ninth day, when the eruption was out, he said he was quite well, but his incoherence made his friends send for me. To-day, J. T— is quite sensible; skin temperate; tongue moist; bowels quiet; pulse 70; still irregular; complains of troublesome dry cough. I have ordered,

Vesicat sterno, and *quininæ sulph.* gr. ij; *acidi sulph. dil.* ℥ ij; *ex. inf. rosæ co. ter. die;*

and, I trust, we have saved him, though, according to the subsultus tendinum, and irregular pulse, Sydenham had signed his death warrant.

CONTAGION.

The question naturally arises now, Is this disease epidemic or contagious? It appears to me to partake more of the character of the former; but after giving you as defined an account as is in my power of the nature of each, I shall leave you to determine, after further investigation, to which class it belongs.

Infection is generated in two ways,—by a disorder being either epidemic or contagious.

That there is an epidemic constitution of the atmosphere, is proved by certain disorders becoming *suddenly* prevalent, and again dying away and disappearing, though the same unrestrained intercourse be carried on in society; which shows that they differ from contagious ones. Not but that coming in contact with those who have the epidemic may render persons more likely to take it, though this is not attributable to any contagious principle in the disease, but because confined air, unpleasant effluvia, and fatigue, or apprehension, by depressing the nervous system, render it more liable to succumb to that conformation of miasma in the atmosphere which constitutes the epidemic, and which will exert its influence over a person under such circumstances, who might have escaped, if all predisposing causes had been avoided; but fear, overexertion, any bad smell, totally unconnected with the disorder, would have the same effect. Ague, cholera, influenza, and acute dysentery, are epidemic.

Contagious disorders seem to be more engendered by the actual organisation of the animal constitution, more partial in their attacks, more limited in their range; but, like epidemics, more communicable to enfeebled constitutions, either by contact, or inhaling the air surrounding the sick, in either case, the *materies morbi, whether it be ichor or effluvia, coming in contact* with the person it infects. In this class I think we may consider itch, syphilis, small-pox, measles, scarlatina, typhus fever, &c.; but even this class, with precautions of cleanliness and ventilation, is not to be dreaded by those who are in perfect health, or those who merely have passing communication, as a physician, or by nurses, who are too actively employed to have time for apprehension. The effect of fearlessness is evinced by the numerous cases in which the affection of a mother, while attending a sick child, overcoming all other sensations, protects her, even through fatigue, from infection. I have known a young lady attend another, night and day, through the worst typhus I have ever seen any one recover from; nay, I ascertained that, in the night, she had moved her, and lain down and slept in her place, in the very lair of typhus,

without being infected; but this lady was totally fearless, and had a well-organised nervous system, as was shown by her being able to *sleep*. The affection which caused her attendance, was not accompanied by that morbid sensibility which weakens the frame, and exhausts its powers. Disorders that are contagious, sometimes visit us epidemically; but I do not consider epidemics contagious; the Plague seems to me to be both, in its nature; Gout, Rheumatism, Pleurisy, &c., neither one nor the other; but in all the different kinds of disease, it is a disregard of cleanliness, the concentration of noxious effluvia, a constitution previously enfeebled by dissipation, over exertion, or neglect of exercise, that aggravate the mischief, and are to be deprecated and avoided.

EFFICACY OF

DIGITALIS

ON THE

TREATMENT OF EPILEPSY,

To the Editor of THE LANCET.

SIR,—Having experienced your liberality in giving insertion in your valuable publication to any communication calculated to advance medical science, I beg leave to trouble you with the following cases, which have fallen under my care within the last three or four months, two of which are rather singular and curious; but I shall not trouble you with several ordinary cases of cure which occurred to me within that period. I remain, Sir, your obliged humble servant,

Jan. 1832. P. SHARKEY,

A.M., M.D. T.C.D., &c., Lic. King and Queen's Coll. Phys. in Ireland, Sen. Phys. to the Cork Gen. Dispens., &c.

CASE I.

On the 25th of July, I was called to see Miss Scott from London, about the age of thirty, rather of the melancholic temperament; general health pretty good, but had a great deal of hepatic congestion; she was subject to epileptic paroxysms for five or six years, without any apparent cause; no mal-conformation. She had one attack in her infancy; her attacks were well characterised; had always some premonitory sensations indicating an approaching fit, which gave her time to repose on a sofa or bed, but no aura epileptica; made water involuntarily in the fit, but no consciousness of it when the fit was over; memory not impaired; the disease not hereditary; pulse regular; bowels disposed to be costive; temper cheerful; a blackness under the eye was usually the forerunner of a fit; had taken great quantities of medicine from various physicians in London, and on the continent, without effect; had been bled at the arm, leeched, cupped, &c., to a great amount; sometimes half-starved, sometimes liberally nourished, and complained of great weakness; not much appearance of abdominal congestion; menstruation regular; fits growing more frequent and severe.

I kept her under the preparatory treatment for five weeks, having discovered manifest indications of hepatic derangement; I got away an immensity of saburra, chiefly consisting of ill-conditioned biliary secretions, highly offensive, of darkish and green colour, with scybala, and morsels of sebaceous matter resembling frustules of undigested potatoes. When I had ascertained that I had cleared her liver and intestines of their foul contents, I gave her four ounces of the infusion of a quantity of the dried leaves of digitalis, which I regulated according to the equivalent adjustment of them to the succulent leaves, as mentioned in my former communication to you, taking into consideration age, sex, constitution, and state of preparation, &c.; the infusion was made by macerating the leaves in cold porter for seven hours, although I intended it to be in boiling porter; but the apothecary committed a mistake; and I had reason afterwards to be reconciled to it, because it gave me to understand that the power of the plant was not lessened by the cold maceration.

I should have mentioned that she had a fit the day before I administered the antidote; and it being the first time of my exhibiting the infusion of the dried leaves, I changed the mode of exhibition, through caution, and directed her to take a tablespoonful every two hours until she should vomit, and then to stop until I should see her; I visited her in the afternoon, and found that she had taken all but one dose, and had vomited only once; I ordered her to take the remainder, and she began to vomit freely. I omitted to mention that I added to the dose ten grains of the powdered root of the polypodium quercus. I visited her next morning, and found that she had vomited frequently and severely during the night; the matter vomited was of a grass-green colour, and very copious. She continued to take, by direction, a pint of gruel every two hours, which came up tinged as above; there was great prostration of strength, but the pupil not dilated; pulse about 56, of good strength, regular, and without any intermission; I visited her on the third day; the other symptoms as before, but the vomiting less frequent; continued the gruel; I visited on the fourth day; the other symptoms as before, but the vomiting greatly abated; the gruel

came up untinged; I then allowed her some light chicken broth; complained still of great weakness; no cramps from the beginning; visited her on the fifth day, and found her recovering her strength; visited her a month after taking the infusion, and she has had no fit.

October 4. Visited her. She has perfectly recovered her health, and has had no fit.

Dec. 3. Visited her several times, and she has had no fit.

Observations.—It is very remarkable that the pupils were not dilated, nor the pulse irregular or intermittent; whether this is owing to *idiosyncracy*, or *constitutional tolerance* of the medicine, or whether it was because of the absence of some principle which could not be taken up by the infusion, I know not; but it is important to know that the cure was effected notwithstanding.

December 8, 1831.

CASE II.

The following case is taken from my notes:—

Sept. 22, 1831. I was called, three weeks ago, to visit Mr. W. Parker, living on the Brickfields Road, who was labouring under epilepsy. Age about thirty, of melancholic temperament; he had been very dissipated in his youth, and was much broken down by the use of large quantities of mercury; he was pale and emaciated; pulse languid; appetite good; had great intellectual lebetude, and dulness of perception. I was often obliged to ask the same questions several times before he would give me an answer, though he seemed to understand the questions; his answers were somewhat incoherent; and he was affected with some degree of paralysis of the tongue and other parts of his body. He complained of occasional headachs, and seemed much depressed by domestic disquiet and commercial embarrassment; his fits of epilepsy commenced six months before; had considerable hepatic congestion; his fæces were of a dark-green colour, and the odour highly offensive; they were frothy and scybulous. I kept him upon the rigorous preparatory system for a month or six weeks.

Sept. 19. He had six severe epileptic attacks in the course of this day. I ordered him four ounces of the infusion of digitalis, made with hot beer, adding to it ten grains of the powder of polypodium quercus. I directed him to take it at nine o'clock in the morning of the 20th; but at three o'clock of that morning, and seven hours before he took the draught, he was seized with violent mania, and could not be restrained without great difficulty; however, they prevailed upon him to take the draught; and he refused to drink gruel after it as usual. I visited him in the afternoon of the 21st, and found him outrageous; he had had no vomiting; pupils not dilated; his attendants found it necessary to hand-cuff him; his pulse was full, and 140 in a minute. I was alarmed at the aspect of affairs, especially at his not having vomited. I accordingly ordered an emetic, with fifteen grains of hippo, and one grain of tartarised antimony, which he refused to take. I then ordered him a table-spoonful of strong table-mustard in a cup of warm water, which he also refused. I then used titillation of the fauces with a quill to no purpose, though I introduced the quill into the stomach; he refused all food, passed a troublesome night, and had about an hour's sleep.

22. I visited him this morning; they prevailed upon him to take a small portion of the hippo emetic, upon which he vomited about eight ounces of fluid of a grass-green colour; he had taken one teacup-full of gruel. On my visiting him I found him somewhat more tranquil; pupils not dilated; pulse 56, irregular and intermittent. I ordered his hair to be taken off, and a tepid evaporating lotion to be applied constantly.

23. Visited him; had no vomiting, purging, nor increase of pain; had a very restless night, and continues very turbulent; is still hand-cuffed; pulse intermitting and irregular, 56 in frequency; pupil obedient to light. Ordered to the Lunatic Asylum.

Observations.

It is curious that in this case as in the last, the infusion produced no vomiting, but had the usual effect upon the circulation and upon the eyes. I hesitated much whether I should venture upon giving the dose, suspecting from my examination of him that there was some disorganisation of the cerebral and cerebro-spinal system, which inclined me to despair of him, and even to apprehend that I might cause additional mischief by the violent convulsion of vomiting, which I expected to follow. I made inquiries about him at the Asylum on the 7th of October, and found that he had sunk into a melancholy state, but had had no fit; he was released from the Asylum soon after, and had no fit. I again visited him at his house on the 31st of October, and found that he had continued free from fits, and had been restored perfectly to his reason; and on the 20th of November he came to visit me and return me thanks; his intellectual powers appear to be as perfect as ever.

CASE III.

Nov. 1, 1831. About four months ago I was called to visit Denis Fitzpatrick, living

on the Sand Quay, aged about 25 years, and of a melancholic temperament; neither abdominal nor hepatic congestion; he has been afflicted with epilepsy for upwards of six years, but with increasing severity for the last eighteen weeks; has had six fits; on some days; his reason is impaired; amaurotic since the commencement of epilepsy, and is now almost blind; is very eager for whiskey, which he says raises his spirits, but which I strictly forbade to be given; is also affected with eneuresis, for which I prescribed uva ursi. Having kept him under the preparatory treatment for several weeks, I gave him the draught about two months ago, but he neither vomited nor purged; the pupil insensible to light, but the pulse was reduced from 80 to 56; regular. On the 30th of October he had one mild fit, and his intellect is improved, which was previously very much impaired. On the 8th of Nov. he had one fit, and I gave him a second draught at eight o'clock yesterday morning, after which he danced about most merrily, and said he felt himself very light and comfortable; but in the course of the day he had two fits more, and vomited twice. Nov. 9th, I found him affected with great prostration of strength, and pulse almost extinct; has a very sharp appetite, and is still craving for whiskey; he had no fit from that time till the 21st of Nov., when he had one fit.

Nov. 24. Has had no fit; eneuresis continues.

Dec. 3. Had two fits three days ago; I gave him six ounces of the common officinal infusion this day, which was his third dose.

9. The last dose produced neither vomiting, increase of water, nor irregularity of pulse; had one fit yesterday.

Observation.

It is quite evident from this case, that where there is cerebral disorganisation there is nothing to be expected from this medicine, nor indeed from any other that I know; but from the experience which I have of this plant, I hope I shall be able to divest it of those frightful consequences which have heretofore been occasioned by its injudicious administration, and thus to confer upon suffering human nature one of the greatest blessings that medicine can bestow.

ON THE

TREATMENT OF CHOLERA.

To the Editor of THE LANCET.

SIR,—I have been daily in expectation of seeing in the pages of your widely-diffused Journal some short and practical observations on the subject of the treatment of cholera, as it has appeared in England; and above all, I had hoped to have been enlightened by the results of the practice of some of the men who have hundreds of cases passing through their hands. The disquisitions are chiefly as to whether the disease is Indian cholera or continental cholera, and as to its contagious nature; but the subject most interesting to mankind seems to be given up in despair—I mean the treatment. I have just had an interview with a gentleman highly qualified to judge of the phenomena of disease, who has visited the field now under the ravages of cholera, and I regret to say that the practice he details as being adopted, appears to me totally inadequate to the cure of the disease, notwithstanding what has been published by Kennedy, Lefevre, Bell, and others, and your own valuable article of the 19th of November last.

I know no means so quick of communicating with the various medical men in the north, or indeed wherever cholera may come, as by your pages; and I hope by offering some physiological reasons, to obtain attention to the practice already recommended by those who have had experience in the disease, and the results of whose experience I fear have not had their due weight, from not being accompanied sufficiently by the *ratio medendi*.

The remedies proved to be most useful are, venesection, tincture of opium, and opium, brandy, and other spirit, essential oils, neutral salts, and emetics of ipecacuanha, or mustard, &c., with external warmth. The phenomena of cholera show congestion, or determination of blood, along the intestinal canal, and a want of circulation in the surface of the body and extremities; bloodletting is the direct means of bringing the current to the surface. Again; there is, from the capillaries of the intestines, a great discharge of watery or serous fluid, separated from the blood with which they are overloaded. How are you so likely to diminish this, as by diverting the current of the blood from them to the surface? Again; by this discharge of serum, whilst the vomiting prevents fluids from being retained, the proportion of red particles becomes too great, and the blood too thick; it is well known that by bloodletting this disproportion will be counteracted, for the absorbents always set to work to keep the blood-vessels full, as we see the blood too thin in persons who have been often bled. Again, as to heat, we know that the surface is warmed by arterial blood coming to it, but in cholera there is a want of circulation from the debility of the heart; it is not able to send on the blood; but if we

draw blood from a vein, then the current will actually entice the blood from, and relieve the heart; and as the blood circulates the patient will most assuredly become warmer. But, it may be said, you cannot get blood from a cholera patient in the severe cases. Perhaps so, if tried too late, but it is difficult to say when it is too late; a friend of mine in India has succeeded in getting blood to flow freely at last, though—after the vein had been opened in both arms, and the surface rubbed and fomented with hot flannels for more than an hour—it began by trickling only in drops, and the patients recovered. You may press blood from the veins of a fresh corpse, and certainly from the veins of a living person, even when no pulse is perceptible, and friction will greatly assist in restoring circulation, as when a person has been frozen or drowned.

Now as to medicine, it is thought that the means recommended and found successful are most contradictory, but physiology will reconcile the apparent difference; one object is, to stop the effusion from the weeping capillaries of the bowels. Laudanum and brandy are very astringent, as well as being stimulant in assisting the heart; but some succeed by giving salt and water, which also has an astringent effect on the mouths of the capillaries, though not stimulant; so has the ipecacuanha and mustard, which others give as an emetic; and so has oil of turpentine, a cheap, ready, and valuable medicine. I will not add any further remedies, because what I have said bears on the practice generally understood, and more might render the subject less directly available. As to external heat I should of course resort to it, but I think it a secondary consideration when we know how soon arterial circulation will heat patients, in spite of all we can do to keep them cool in many diseases.

In my opinion giving half-grain, or grain doses of opium, with calomel, a practice which is efficacious in mild diseases, is totally inadequate to a disease of such tremendous power as cholera in severe cases; we know this quantity of opium will check mild cases, but when we consider the insensibility of the parts, and the medicine being rejected by vomiting, or washed away, or diluted by the watery evacuations, laudanum and brandy must be given, in from five to ten times the ordinary doses.

I have only one argument more to use. There are many cases in which the practitioner, or even any old nurse, who is conversant with the disease, will say at once—This person must die according to the ordinary course. Pray then, I should say, as you know they can but die, try to get a little blood from them first, and perhaps you may hear them tell you, "I begin to feel warm in the back," and see them snatched from the grave.

A. Billing, M.D.

January, 1832.

DR. CLANNY'S

MODE OF TREATING

THE FOUR STAGES OF CHOLERA.

Having laid my discoveries and views of the proximate cause of epidemic cholera before the public, in an outline published in The Lancet of the 7th of this month, and in December last given the manuscript of my work on cholera to the printer for immediate publication, and of which, from a pressure of business, only two sheets have been printed off, I consider it my duty (now that we have removed the disease from the town, and having a little spare time) to make public an epitome of my practice, founded upon my analysis of the blood and fluids of cholera patients. I divide the disease into four stages.

1st Stage.—If called in at this stage, which I call the first collapse, we find the blood and lymph have left, to a great extent, the surface of the body, and that the centre of the system is overloaded thereby. In order to empty the stomach and gall-bladder, and to equalise the circulation and determine to the surface, I prescribe the following emetic powder,—

℞ *Ipecac. pulv.* ℈j;
Antimon. tart. gr. iij. ℿ.

The patient should be placed in bed, and dry heat and frictions, by three or four persons, to different limbs, continued, till perspiration be brought on. Venesection, to the extent of 15, 20, or 25 ounces, should be used. This will tend to augment the perspiration, as well as to relieve the centre of the circulation, and cause a more ready flow of blood in the branches of the circulatory system. The patient should have barley-water, toast-water, or the like.

2nd Stage.—If called in at this stage, during the vomiting and purging, or both, the above plan of heat and frictions should be had recourse to, and the following enema, at blood-heat, instantly administered,—

℞ *Opii pulv.*, gr. vj;
Aquæ puræ, ℥iij; *solve*; *ft. enema*;

to be retained in the rectum by a long tapering cork and a T bandage; the cork to be oiled with olive oil. This plan I have used with enemata, from seeing, several years ago, an account published in the Medico-Chirurgical Transactions, of a person from the Netherlands, who at the time was an inmate in one of the London hospitals, hav-

ing used a wooden plug "instead of a cork, as customary in his country, to cure diarrhœa." In one, two, or three hours this enema may be removed, never omitting to use the cork, and augmenting or diminishing the quantity of pulvis opii as needful. No inconvenience ever arises from using this plug. By this plan the purging and vomiting will be arrested. The patient should use lemonade, or orange juice, in barley-water or toast-water, or a little very weak chicken-soup; but no other food, and no medicines by the mouth.

3RD STAGE.—If called in at this stage, we find that the patient has now vomited and purged, till, in a direct manner, the serum of the blood is almost all expended. I order what we call at Sunderland mutton tea, viz., a strong infusion of lean good mutton (the water for the infusion as hot as possible) avoiding salt or spices.

By means of a syringe or bladder and pipe, at a proper temperature, this infusion is thrown in a careful manner into the bowels, and there retained by a cork as above described. From this the patient will feel instant relief, and the symptoms will be immediately alleviated. Should the desire to get quit of the injection distress the patient, let it pass, and in half an hour renew it; by this means keeping a pint of the infusion always in the bowels. In some cases it may perhaps be needful (though it was never so with me) to add to the enema six or eight grains of pulv. opii.

In every instance I have observed that when the enema was returned it was tinted with fresh bile, and of this the quantity and quality were generally augmented and improved till fæces made their appearance. About this time the kidneys secrete urine, and the bladder readily expels it, at first in small quantities, and soon afterwards in abundance, in which there is to be found the usual salts without albumen or fibrin. External heat, pure air, frictions, whether there be cramps or not, should be diligently employed, attending of course to the symptoms, and obviating the continued disposition to collapse, so that the circulation on the surface may be kept up. The patient should be kept comfortable in every respect, and his mind made easy, and even, if possible, cheerful.

The attendants should never leave him for a moment, for should he sit up, he might die from the weakened state of the heart and arteries, or from the thick and effete blood, or from both.

The medical attendant should never lose sight of his patient, as at this stage and the following everything depends on sound knowledge and prompt measures.

Should the patient now desire nourishment, he may have a little tapioca or sago, much diluted, to which a small portion of genuine brandy or sherry may be added.

I have always found that, at this time, all the peculiar symptoms of cholera leave the patient. Of course cupping, leeching, and blistering, may be had recourse to, so as to remove topical affections or particular determinations; and, in my opinion, all medicines by the mouth are worse than useless up to this time of this stage. Should the liver not readily secrete bile, nor the kidneys urine, from ten to twenty grains of blue pill may be given every four or six hours till these secretions commence.

In general I have found this to answer, but from two to twenty grains of calomel may be, under certain circumstances, substituted for the blue pill in the same manner, to which a small portion of pulvis opii may be added. Effervescing draughts have often been most useful in my practice.

4TH STAGE.—At this, which is the sequela or consecutive stage, or fever, of the disease, we find many symptoms in common with the last stage of typhus fever, which from my analysis of the blood, in my treatise on typhus fever, I find to correspond in many points.

At this stage a similar plan to that recommended in the above work should be had recourse to. The greatest care is needful to avoid inflammatory symptoms, and to remove them should they occur, never for a moment forgetting, that in this sequela, the blood being surcharged with free carbon, which, from the first, induces the greatest debility, as well as vomiting and purging, patients often die of debility alone.

In the rapid type, as the disease runs through all these stages in a few hours, all the energies and talents of the medical attendant are imperiously called for; indeed the medical man should not leave his patient for ten minutes, for should the patient fly to cold water, or get out of bed, or even sit up in bed, his life would be compromised.

W. REID CLANNY, M. D.

ANASARCA.

ALLEGED DEFECTS IN THE CYCLOPÆDIA OF MEDICINE.

To the Editor of THE LANCET.

SIR,—On taking up the *Cyclopædia of Practical Medicine* recently published, I perused with some attention the article on anasarca, which I perceive is written by Dr. Darwall of Birmingham, and was surprised that some omissions had been made, which seemed to me of much importance. Whether this has arisen from want of room in which to discuss every part of his subject or not, I am unable to tell, but from

the talent displayed in several parts of the paper, it cannot be supposed that the author is ignorant of the many important facts connected with the disease, which he has neglected to mention.

This gentleman has alluded to the frequent production of anasarcous swellings from disease of the heart, and has brought forward cases illustrative of this; but he has omitted to take notice, amongst other causes of dropsical effusion, of organic diseases of the liver and kidneys, or of the symptoms by which these may be detected. And it appears to me rather singular that the author should have only cursorily spoken of the state of the urine in anasarca, and only in reference to its supposed use in the diagnosis of inflammatory and asthenic dropsy, at first spoken of by Dr. Blackall, when it is perfectly well known, and must of course be fully so to the author, that in anasarca, when the kidneys are diseased, the affection may always be detected during life by the altered condition of the urine. Indeed, from the positive assertions of Dr. Bright, in the first volume of his *Medical Reports*; of Dr. Christison, who published some years ago an able paper in the *Edinburgh Medical and Surgical Journal*, containing the analysis of the urine and blood in several cases, and of Dr. Gregory of this place, who has paid very great attention to this subject, and has published his opinions in the two last volumes of the above periodical, accompanied by a profusion of cases, it is established beyond doubt, that in cases of dropsy, when the urine is of a pale or brown colour, of low specific gravity, when it coagulates by heat, by different acids, corrosive muriate of mercury, and other tests of albumen during life, a *post-mortem* examination invariably displays a peculiar disorganisation of the kidneys; for in no one instance in which the urine has been found to possess these qualities, have these organs been found in a healthy condition after death.

The most decided test of the albuminous impregnation of the urine is, heat, and this may be further confirmed by the addition of different reagents. Corrosive muriate of mercury, though the most delicate test of albumen, is generally decomposed in healthy urine, and is precipitated of a slightly darker colour than the pure white precipitate from diseased urine, and is evidently a different substance, although the contrary is believed to be the case by Dr. Gregory,* from a difference in their properties, and their agreeing in no one respect in their solubility in a variety of chemical agents.

The occurrence of urea in the blood of anasarcous patients from different causes, and who have at the same time organic disease of the kidneys, is not in the slightest degree touched upon, nor of course are there any conclusions drawn from this remarkable circumstance. I have in my possession at present some of the nitrate of urea, formed from the blood of a girl who died anasarcous, which has all the characteristic properties of that from healthy urine.

Having made these desultory remarks, I beg to express a hope, that in the next edition of the Cyclopædia, Dr. Darwall will be allowed more space for the full exposition of his subject, or that the writers on ascites and dropsy which have yet to appear, will profit by the omissions which have been made in his paper. I have the honour to be, Sir,

Your obedient servant,

W. P. Bain.

Edinburgh, 19th Jan., 1832.

* Edinb. Med. and Surg. Journal, No. 110.

EXPERIMENTS

TENDING TO PROVE THAT

MURIATE OF SODA

IS NECESSARY IN THE CHANGE WHICH TAKES PLACE WHEN

VENOUS BLOOD BECOMES CONVERTED INTO ARTERIAL.

By Isaac Lyon, *Esq., M.R.C.S.L., Hull.*

The following detail of experiments (made in May and June 1830), in my opinion, tends to prove that for the venous blood in man to become converted into arterial, it is not only necessary that oxygen should be present, but that the serum should contain muriate of soda.

Exp. 1.—I took some coagulum and exposed the lower surface of it, which was then of a dark-purple, to the air, leaving it in its own serum. In the course of twelve hours the surface which had been of a dark-purple, was converted to a fine scarlet.

Exp. 2.—I took some coagulum, and reversed it as in the former experiment; but instead of leaving it in its own serum, I poured it off, and substituted in its place the common spring-water of this town. After the lapse of thirty hours no change had taken place. I now poured off the water and returned the serum, in fifteen hours it became changed to a fine scarlet, so that from this experiment we may fairly conclude, that without the serum the change of colour cannot take place, and therefore that the serum is necessary for this change. These experiments I have frequently repeated, and invariably with the same re-

sult. Leading to the same conclusion are the following experiments.

Exp. 3.—I took some coagulum, poured off the serum, and replaced it with distilled water. After having remained five days thus exposed, no change had taken place.

Exp. 4.—I took some coagulum, reversed it, and put it into rain water sufficient to cover it. In seventeen hours no change.

Exp. 5.—I took some coagulum of the blood from which the rest had been taken, reversed it, and left it in its own serum, sufficient to cover it. In seventeen hours it had become changed to a scarlet.

Exp. 6.—I took some coagulum, and left it uncovered in a saucer without any fluid, having previously dried the surface with a cloth. In twenty-seven hours the edges near the saucer, from which a little serum had exuded, were partially changed to a scarlet, the remainder of it, when dry, was of a dark-purple.

Having thus, I think, proved satisfactorily that the serum is necessary for this change, I next endeavoured to ascertain whether every substance entering into the composition of the serum was necessary or not. With this view I imitated the composition of the serum, and in the

Exp. 7.—I put some coagulum into some water, having three drachms of the white of egg mixed with it. In forty-six hours no change.

Exp. 8.—I put some coagulum into four ounces of rain-water, holding in solution three drachms of the white of egg, and four grains and a half of the subcarbonate of soda. In sixteen hours it had even become paler.

Exp. 9.—I put some coagulum into two ounces of rain-water, holding in solution two grains and a half of phosphate of soda. In four hours no change.

Exp. 10. I took some coagulum, and reversed it in four ounces of rain-water, holding in solution three drachms of the white of egg, nine grains of muriate of soda, four grains and a half of subcarbonate of soda, and four grains and a half of phosphate of soda. In four hours it had become changed to a scarlet. From this experiment we may conclude that muriate of soda is necessary. In order to prove this more satisfactorily, I made the following experiments:—

Exp. 11.—I reversed some coagulum in six drachms of rain-water, holding in solution two grains of muriate of soda. In sixteen hours it had become changed to a fine scarlet. From this experiment we see clearly what it is that assists in the change, viz., the muriate of soda.

Exp. 12.—I reversed some coagulum in distilled water, holding in solution white of egg and muriate of soda, with the same effect as in the last experiment. These experiments I have frequently repeated, and with the same result, except in one experiment, where I found no change; but as the experiments in favour of the muriate of soda being necessary are numerous, and as I find only one experiment against it, I think we must arrive at the conclusion, that for the venous blood to become converted into arterial, it is necessary that the serum should contain muriate of soda. Hence I shall endeavour to draw certain inferences.

1. That common salt is absolutely necessary as an article of diet. In confirmation of this I may state a fact mentioned, I think, in Parke's Chemistry, that criminals in Holland were formerly destroyed in the following way, by not allowing them any salt with their food.

2. That too much salt will be injurious, by causing the blood to be too highly oxygenated; for it appears to me to amount to the same thing, whether too much oxygen be supplied to the system, or the system rendered too apt to be acted upon by the oxygen contained in the air we breathe; for, from the experiments of Broughton, it appears that too much oxygen is injurious to the system.

3. That the reason why certain food predisposes to the rot in sheep and other animals, may be, that it does not contain sufficient muriate of soda, and that hence arises the use of mixing salt with the food taken from land which causes the rot. I wish some eminent chemist would examine the substances grown upon land which produces the rot, and inform us whether this idea is correct or not.

4. That muriate of soda may be of use in some diseases where a due oxygenation of the blood does not take place. Upon this point I intend making some experiments on man, and shall send you the result, if found correct.

5. As, from the different accounts hitherto received of the Russian cholera, it appears that the blood taken from the patient is very black and not properly decarbonised, may not the administration of muriate of soda be useful in assisting in this decarbonisation? I am sorry that some eminent chemist was not joined to the deputation sent from this country by our government to report upon the cholera, as I conceive that if the blood and ejected substances had been chemically examined, they would have afforded us great information, and assisted towards the proper treatment of this formidable disease.

Hull, Dec. 1831.

DR. STEVENS AND DR. JOHNSON.

REJOINDER OF DR. STEVENS.

To the Editor of THE LANCET.

SIR,—It is painful for me to be compelled to appear so often before the public, on purpose to resist the unfounded attacks of an individual, who is no sooner fairly convicted of one misrepresentation than he tries to evade it by inventing another.

The chief accusation contained in Dr. Johnson's last letter, is one which concerns himself fully as much as it does me; for in it he asserts, that the *analysis* of my paper on the blood, which appeared in his journal for July 1830, was written by myself. Now, there is but one charitable construction that I can put on this, and that is, to consider it as a modest intimation to the public, that any author, who wishes to review his own work, will find a place for the same in the *Medico-Chirurgical Review*, of which this accommodating individual is the sole editor.

It is very natural that a certain class of authors might wish to be accommodated in this kind of way, and lately there has been but too much reason to fear that such accommodation is not uncommon; let this, however, be as it may, there can be no doubt that the editor who permits such conduct is just as culpable as the author who wishes to be the writer of his own review. I am sorry to observe, that Dr. Johnson, in the present instance, has brought an accusation of this kind against himself; but in as far as relates to this instance I will stand his friend, and try to prove that he is not guilty.

He asserts that, as he had not heard my paper read at the College of Physicians, I had called upon him, and requested that he would insert a report, or analysis, of the said paper, written by myself. "This," says he, "I did publish, and this is the unfair statement by which he (Dr. Stevens) says I wilfully endeavoured to mislead the public." As Dr. Johnson has fortunately furnished us with a great variety of polite expressions, I will borrow one of his own, and aver, that there is scarcely one iota of truth in all this. I was not present myself, neither did I know whether Dr. Johnson was or was not present when that paper was read; nor did I care. Dr. Johnson's journal may be, like others of the day, a useful medium for disseminating original facts, but those will be sorry to have the doctor himself on their side who have either read his own works, or who know the light in which his *opinions* are considered by the *intelligent* part of the medical profession. It is true, however, that I did call upon him, but not for the purpose he has specified.

Some of your readers may recollect that, in 1814, I published an account of an operation in which I had tied the internal iliac artery in 1812; the patient died in the year 1822, and the parts were preserved. Dr. Van Brackle, who had been present at the operation, and who was also present at the examination of the body after death, wrote a minute account of the preparation, and sent it to his friend Dr. Chapman (the then editor of the Philadelphia Journal), which was published in that work, in one of the numbers for 1822. Believing that this would find its way into the English journals I did not write any account of it myself, but I kept the preparation in my possession and brought it to England with me, when I returned to this country in 1829. And the truth is I considered the preparation at that period as of little value: for I thought the circumstance of my having tied that artery was universally admitted. But in this I was deceived, for, in May 1830, in looking over Mr. Lawrence's lectures, which had then just been published, I found that he had there expressed a doubt about the correctness of the statement which I had published. In consequence of this, I waited on Mr. Lawrence, told him that I had the preparation in my possession, and expressed a wish that he would examine it as early as possible. The preparation was immediately sent to the museum of the College of Surgeons, where it was examined by Sir Astley Cooper, Mr. Travers, Dr. Hume, Mr. Lawrence, and many others, who expressed themselves perfectly satisfied with respect to the correctness of the original statement which I had made.

A few days after this, I received a communication from the Secretary of the Medico-Chirurgical Society, requesting me to draw up a description of the preparation, and to send it to them for the next number of their Transactions. This, however, I could not do, for I was then on the very eve of embarking for America; in consequence of which I requested Mr. Owen to do it for me, and promised to procure him the description which had formerly been made by Dr. Van Brackle, who had seen the parts in their more recent state. On purpose to procure this, I applied to several of my acquaintances, but none of them could give me the volume which I wanted. Amongst others, I went to my friend Dr. James Clark, who told me that Dr. Johnson was the most likely person to be able to give me the journal in question; and, as I did not know Dr. Johnson myself, Dr. Clark gave me a few lines to him, saying who I was, and what I wanted. It was for this purpose that I called upon Dr. Johnson, and not as he

avers, to request that he would insert in his journal an analysis of my paper on the blood written by myself.

The said paper had been read at the College of Physicians a short time previous to this period, and during this interview it became the subject of conversation, but who introduced it I do not recollect. I will allow, however, that it was probably myself, for I felt then, as I do now, an anxious desire that the facts contained in that paper should be made as public as possible. In the same conversation, Dr. Johnson expressed a wish to get a copy of the said paper, on purpose to be able to lay a correct account of the facts which I had stated before his readers. I told him that I would send him a correct copy, provided he would give me a positive promise not to publish it in its original form; for, as I believed, the College of Physicians retained to themselves the right of publishing any paper which had been read at their meetings. In consequence of this conversation I afterwards sent, or gave him, a correct copy of the original paper, and, in as far as Dr. Johnson has given a statement of the facts contained in that document, they are perfectly correct. He states, however, what is not correct, when he asserts that he can prove that the analysis, and the only statement which he had ever given of my views, were written by myself. But I have only to refer to the very first sentence of the said analysis to convince any impartial individual that I, at least, could not have been the writer of that paper. He begins by stating, that "At a late meeting of the College of Physicians, a paper on the above subject *was read by Dr. Stevens*, who has for many years been a practising physician in the West Indies.

Now in the first place, so far from having read the paper myself, I was not even present at that meeting, and, in the second place, you will observe that even my name is mispelt, which is not likely to have been the case had I been the writer of that article.

It is customary, I believe, for an author who has written a work, to send a copy of it to the editors of journals. I have explained how and when Dr. Johnson got possession of a copy of mine. There is, however, a wide difference between an author giving a copy of an original paper to the editor of a journal, and writing a review of his own work.

Dr. Johnson asserts, however, that he has a document in his possession which will prove that I had written the analysis myself. Now if he has such a document, or any other of mine, except a correct copy of the original paper on the blood, let him bring it forward and expose it to a fair inspection within fifteen days from the publication of this, and then I will prove either that the said document is entirely a forgery, or confess that my memory has deceived me sadly in this affair. But if he does not produce it within the above period, I shall consider his assertions about this document to be as untrue as the rest of his other misrepresentations, and then leave him to God and his own conscience.

I have stated in my former paper, that I had mentioned to Mr. Greatrex my belief, that the aërial poisons which cause fever, have an acid effect in blackening the blood, but that the alkaline or neutral salts possessed the power of again restoring the red colour of this blackened fluid. Now I can readily conceive how Mr. Greatrex, with whom I had merely a verbal communication on this subject, might give an incorrect view, without any intention on his part of misrepresenting my theory. I can also believe that Dr. Hacket, whom I have never seen, might easily fall into the same error; but I cannot make the same excuse for Dr. Johnson, who had previously given a fair view of my facts and opinions in his own journal, and after having published in his own work a correct statement of my views with respect to the effects of salts and alkalies on the blood. I say again, he had no right, either in the Westminster Society, or in his letter to you of the 5th December 1831, to put insufferable nonsense into my mouth, which he must have known I never could have uttered, or to make me the supporter of opinions which he himself must have been perfectly aware were in direct opposition to my own facts.

Now, notwithstanding all his sophistry, the following is the fair statement of the case. In his journal for July 1830, Dr. Johnson published a correct statement of the outline of the facts contained in my paper. At that period he spoke favourably of my opinions, and represented "the means which I had proposed for combating a dreadful scourge of the human race as both simple and practicable." Secondly, when he found that some eminent individuals in the profession considered the proposal which I had made for the treatment of cholera* as better than his own, he then got

* I may observe, that though this treatment has not yet got a fair trial in cholera, still, in as far as it has been tried, it has been found to be decidedly the most successful. See the result of this treatment at Sunderland, as well as Sir William Crichton's letter in the supplement of a contemporary journal, in which it is stated, that in an hospital at St. Petersburgh, only three died out of thirty patients who had been treated with large doses of muriate of soda: but of this hereafter. It is probable that the cholera, like the yellow fever, never would have been so fatal had it not been for improper treatment. Water, saturated with carbonic acid, an infusion of dried raspberries, mustard ema-

up in a violent passion, and on the authority of certain papers, uttered a tirade of insufferable trash about soda and sodafied blood which he himself must have known to be incorrect; for, as I have said, the whole of it was in direct opposition to my own facts, such as he himself had previously published in his own journal. It was for this that I accused him of wilful misrepresentation; and, having convicted him of this, even by his own evidence, how does he try to get rid of the proof? why, simply by inventing one misrepresentation on purpose to conceal another; and tries to mislead those who do not think for themselves, by asserting *that he had never made any statement except that which was contained* in his own journal. And though I have admitted the said statement to be correct, yet he labours to prove that this is the ground on which I have accused him; but the truth is, that I never accused him on any such grounds, for I admitted that the facts as stated in his journal were perfectly correct. He asserts, however, that this is the only statement which he had ever given of my views. Now the memory of this gentleman must indeed be very bad, for, independent of what occurred in the Westminster Society, is it not true that in his letter to you of the 5th December he asserts, "that he had in his possession documents from which it appeared that *soda* had nothing whatever to do with the success of the treatment in Trinidad; that *the blood of sodafied patients* after death was found to be precisely similar to that of the *unsodafied*;" and, lastly, he intimates that the success had been owing, not to *soda*, but to the reddening of the blood by the warm-bath.

It was for this that I accused Dr. Johnson of unfair dealing, and wilfully endeavouring to mislead the public; for, as he himself had previously published a correct statement of my views, he must have known that the opinions imputed to me by the Trinidad physicians were directly the reverse of those which I had publicly expressed. Under these circumstances, he had no right to bring forward such documents as affording any proof against either my facts or opinions; and if his having done so be not a wilful misrepresentation on his part, then it is madness; and the person who can be guilty of such conduct, without meaning to misrepresent, is fitter to become the inhabitant of a lunatic asylum, than to be, as he professes, an instructor to the public.

Dr. Johnson denies having had any previous communication with the writers of the Trinidad documents, and this perhaps is true. I did not however directly accuse him of corresponding with either of these gentlemen; I observed, "that Mr. Greatrex, apparently in answer to a letter from Dr. Johnson, states, &c." and I did so for the following reason: If you will refer to page 296 of the last number of Dr. Johnson's journal, you will find it there stated, that "the following extract from a letter from Mr. Greatrex (*after perusing a communication of mine on the foregoing subject, and essentially* the same*) I beg leave to append, &c."

Mr. Greatrex's letter, which is apparently written to Dr. Johnson, is dated July 24th, 1831, and in this he states, "that I withheld, as you have seen by Dr. Hacket's letter, all authority, &c." Dr. Johnson, however, observes, that he had "never corresponded with Mr. Greatrex, either before or since." Now, are we to consider this as an indirect admission that he had corresponded with him at that period? And when we find it stated in a journal, "that Mr. Greatrex in answer to a communication of *mine* on the foregoing subject, &c.," who are we to suppose to have been the correspondent in this case but the editor himself? It is evident from what follows, that it was not written in answer to any communication of Dr. Hacket; and if it was not to Dr. Johnson, to whom was it written? It is possible that some other unknown third person has been making himself busy in this affair: and, if so, Dr. Johnson can only clear himself of the suspicion by explaining to the public who this person is.

Dr. Johnson observes that he had acted the part of a friend, and denies that he had made any attack upon me: "for where," says he, "did I attack him at all?" Now, Dr. Johnson is aware, that when my last paper was written I was under an impression that he had asserted, in the Westminster Society, "that he had a document in his possession which would prove that what I had published were forgeries; and that my conduct was disgraceful even to the whole profession." This statement found its way into the *Morning Post*. It is, however, but fair to state, that when I requested a friend to wait on Dr. Johnson, and to require either a disavowal of, or an apology for, such outrageous conduct,

tics, &c. &c. have lately been found to be successful in the treatment of this disease. Not that there is anything specific in these remedies, for they are more useful merely because they are less pernicious than calomel *in scruple doses*, and the other destructive agents that have been recommended by Dr. Johnson, and used by most of his colleagues who have treated cholera in the East.

* From this it would appear, that the said document has undergone certain changes since Mr. Greatrex had seen it; and of this I am very glad, for I can scarcely believe that Mr. Greatrex would have lent his sanction to the improper language which has since appeared in Dr. Johnson's Journal.

that he signed a document in which he affirms "that the above report is totally incorrect, and that he had never made any charge of the kind."

Such being the case, and though the editor of the *Morning Post* still insists that their original report was correct, yet I will wave what occurred at that meeting, and simply refer to his own letter to you of the nth December 1831, in which he states, that he "had documents in his possession charging me with misrepresentation of facts, also with garbling of extracts so as to suit my own purpose, and other grave charges, that were, in his mind, calculated to do injury *and even disgrace to the medical profession.*" Now, if Dr. Johnson has any sense of propriety yet left, let him read this; and then he may himself answer his own question and inform the public "where he had made any attack against me at all."

Dr. Johnson, as the editor of a journal, did perhaps but his duty to the public as well as myself by publishing the documents which he had received from Trinidad; but he had no right, at a time when he had seen only one side of the question, to make himself the partisan of either party, and, nearly a month previously to the publication of the Trinidad papers, to prejudge the case and endeavour to make a strong impression in the public mind against me, by asserting that he had papers in his possession containing grave charges of disgraceful conduct, charges which the documents themselves, *even in their present form, do not contain.*

You will see, by referring to his own letter of the 5th, that he did use language which he had no right to have used; and you know also, that he did make an impression on the public mind which the Trinidad papers themselves never could have made, had they *merely appeared before the public in their original form, and without any previous comment.* Yet, after all this, he has, to use his own elegant expression, "the affrontery" to state that he never made any attack against me, and that he took no part in the controversy, "either *pro* or *con.*" Poor innocent man! He has really been ill used. He took no part in the controversy; he made no attack whatever; he only accused me of misrepresenting facts, garbling letters, and other conduct that was most disgraceful to the whole profession; and then, like a good, simple, honest editor, he had merely published *both* sides of the question, although he is perfectly aware that, at this moment, the Trinidad documents are circulating against me all over the world, and uncontradicted, at least in as far as his Journal is concerned. His next number, however, will show whether he is or is not deserving of the good character which he has given of himself.

I have formerly said, that when my paper was written for the College of Physicians, I had a thorough conviction that *every* syllable in that document was most religiously true, and I believe that neither Dr. Johnson nor his Trinidad correspondents have disproved any one of the *facts* contained in that paper. With respect to my opinions, I trust that the profession will think charitably on this subject until I can find time to bring forward the numerous experiments and facts with which my views will be supported hereafter; and if in the mean time a Prout, a Christison, a Bostock, or a Brande, should find fault with my theory, I will receive their observations with thanks, and try to profit by their superior knowledge; but I will not be schooled, nor occupy any more of my time in refuting the observations of men who argue the propriety of using a hot-bath in the hot stage of yellow fever, or who believe that a neutral salt, and a pure alkali, are one and the same.

If Dr. Johnson had had any knowledge of the subject about which he was writing, he would have seen, at one glance, that the Trinidad documents were a tissue of errors from beginning to end; and if he had had even one particle of fairness in his composition, he would have written to his correspondents and requested them, for their own sake, to contrast the views which they had given of my opinions with those which he had previously published in his own journal. He would have said, "It is evident, even from your own showing, that Dr. Stevens has never advised you to use even one particle of either soda or any of the other alkalies, yet you are constantly confounding the alkalies with the neutral salts, and talking eternally about soda and sodafied blood, while you admit at the same time, that you had never used one particle of either soda, or any other pure alkali, in your hospital." Had he done this he would have delayed the publication, as he himself says he had intended to have done, and by doing so he would have acted the part of a friend to his foreign correspondents; but in place of this, and on purpose to secure a little momentary triumph over opinions which he found to be gaining ground, he gets up in a violent passion, and for fear that any other method of treating cholera should be found to be better than his own, he makes use of the Trinidad documents, to impede for a period the progress of truth, and in place of any reasonable argument, which he was totally incapable of using, he brings forward assertions in place of facts and statements, about soda and sodafied blood, which he himself must have known at the time to be mere folly. "A man may walk the routine of ordinary practice, converse with hysterical wives, and whistle as he walks; but he who has

witnessed the sufferings of such an awful disease, without feeling an anxious desire to understand the cause and alleviate the misery, has neither a head nor a heart worth the caring for."

It is the duty of an editor, or of a professed critic, to lay before his readers a clear view of facts, and, if necessary, to reason from these as fairly as he can. It is one at least of the essential characteristics of a gentleman not to attack the honour of another until he is fairly convinced that he deserves such treatment. I have, however, stated the facts, and whether Dr. Johnson has acted with wisdom towards himself, or fairness towards me, I shall now leave to the decision of the public.

By inserting the above you will oblige, Sir, your obedient servant,

WM. STEVENS, M.D.

Albany St., Regent's Park, Jan. 23, 1832.

EFFECTS OF STRANGULATION

ON THE ORGANS OF GENERATION.

To the Editor of THE LANCET.

SIR,—In reference to several communications which have appeared in THE LANCET on the effects of hanging on the male and female organs of generation, and the sexual functions of the cerebellum, permit me to lay before you a short statement of what has fallen under my own observation in relation to this subject.

In October, 1820, I was accoucheur to a lady who, as soon as she was delivered, anxiously wished to know the sex of the child. Accordingly, in absence of light, I ascertained by feeling with my hand the sex of the infant, and told her that it was a fine boy. But what particularly arrested my attention on this occasion was, a strong penal erection in the child, who seemed to have been almost strangled in the birth. The presentation was natural, and the pains vigorous, and at short intervals; the head was very much compressed, and the labour was finished nearly in an hour. Since my attention has been directed to this subject many opportunities have occurred to me of verifying the accuracy of the position of some of your correspondents. But the circumstances in which the accoucheur is most likely to recognise its truth are, when the labour is vigorous, the head strongly compressed, and retarded for a time in the passage through the pelves, but the child ultimately born without much resistance from the soft parts. Sometimes, however, the rigidity and resistance of the soft parts produce nearly a similar effect. The fact may be noticed in very protracted labours, and in most instances the child's face is turgid and livid, and respiration often does not take place for some minutes after delivery.

It may be now three years since I attended the following case, of a woman, about 30 years of age, in child-bed. The labour was severe, though not remarkably so, for in two or three hours she was delivered of a female child without any particular assistance. It was on the second day after delivery, when I called to see my patient, that she directed my attention to some marks of blood on the child's linen. At first I thought lightly of the matter, supposing the appearance to have been produced by some drops of blood from the umbilical cords. She however spoke significantly on the subject, and talked of it as catamenial. In confirmation of her view of the subject she remarked that this was the second female child which she had had that exhibited this appearance during the first and second day after delivery. This was quite new to me; at the same time I endeavoured by a careful examination to ascertain the source of this singular phenomenon. No fluid blood was discovered, but around the root of the clitoris, some sanguineous matter was found in a dry, granular, form, whence it is presumed the discharge, if it deserved the name, had proceeded. I took an early opportunity of inquiring of a professional gentleman who had been nearly 40 years in the practice of midwifery, if he had ever noticed anything similar to what has been just mentioned, but he frankly acknowledged that he had not. In a conversation which I had on this subject with a midwife of extensive practice in this place, she stated to me that she had noticed the appearance referred to several times in the course of her business. Since I began to make inquiries on the subject, some other cases of the kind have been mentioned to me.

About seven weeks ago I was consulted in a case of apoplexy. The woman had been remarkable for regularity with regard to the recurrence of the catamenia, but while she was labouring under this disease, which proved fatal on the tenth day, the menses appeared on the beginning of the third week, and continued two days, being nearly a fortnight sooner than usual.

In severe epileptic fits, when the face is turgid, livid, and the veins of the head are ready to burst, seminal emissions are, as I understand, not uncommon. Now in all these different kinds of cases the cause of the phenomena seems to be a mechanical effect which is produced on the cerebellum, which, through the medium of the nervous system, becomes apparent in the organs of generation, and bears a striking analogy to the consequences which hanging has upon eunuchs and criminals. I am, Sir,

Jan. 12, 1832. A.*

* This signature has been authenticated.—ED. L.

SINGULAR CASE IN WHICH

A STEEL FORK

WAS

EXTRACTED FROM THE BACK.

To the Editor of THE LANCET.

SIR,—The accompanying singular, and as yet inexplicable case, occurred on board H. M. ship *Belvidera*, while I was attached to that ship in the Mediterranean last summer, and may be deemed worthy of insertion in your valuable Journal. I am, Sir,

Your most obedient servant,

DAVID BURNES, M.D.
Assist.-Surg., R.N.

London, Jan. 23d, 1832.

CASE.

Robert Syms, ætat. 23, a stout little man, complained to Dr. Tweeddale and myself, about the middle of June last, of slight pain about the inferior angle of the right scapula. On examination we found what we then called a small moveable tumour, situated between the scapula and the spine, but close to the base of the former. As the pain was not severe, and there was no other symptom denoting inflammation, or requiring immediate attention, he was desired to do what little duty he could, and to return if he felt any inconvenience. At the end of three days he again applied for relief, having increase of pain, and especially on moving the right arm, the tumour having then the appearance of a common phlegmon in the early stage of suppuration. Poultices were ordered, and suppuration having advanced rapidly, "the boil" was opened the following day, the 19th of June, with a common lancet. The poultices were again applied, and the case attracted no further notice till the 22d, when the report book states,—"swelling less, discharge thin and watery, and on introducing the probe, the edge of the scapula feels bare and rough; continue the poultices."

June 23. A similar report was made, and in addition it was stated, that something black and shining was seen in the wound.

24. Discharge thin; has little or no pain, except from probing. There being evidently some foreign body in the wound, the opening was enlarged directly upwards, and a piece of steel, about the thickness of a common ram-rod, presented itself, but resisted strongly any efforts to extract it. Being unwilling to put him to further pain while there was a chance of its coming away by poulticing, and pulling it with the forceps daily, this gentler course was agreed on in preference to making a further enlargement of the wound. Being questioned as to the nature of the piece of steel, he expressed himself as much astonished as we were at its presence, and said he should not have known it had we not told him, and had he not felt pain from our pulling it with the forceps. He had never been in action, having been only two years in the King's service, nor did he recollect having received any wound by which anything of the kind could have been introduced. About two inches below the opening made on the 19th, we observed a small white speck, or mark, in *size* not unlike that left by a leech-bite, but rather resembling the mark left many years after vaccination, than a cicatrix of a wound. This was the only vestige of anything like a wound that we could detect in his back.

July 2. The poulticing has been continued, and there is now a free discharge from the wound; the steel has been pulled daily by the forceps, and admits now of further motion, especially laterally, but is yet forcibly retained at its upper part; its direction is nearly parallel with the blade of the scapula, close to which it lies, and in its course upwards it seems to incline deep into the substance of the muscles. About an inch of it can be seen when the integuments are retracted. He is averse to further measures; has no pain except from the use of the forceps. Continue the poultices.

16. Though the poulticing has been continued, and the steel pulled daily, there is no material alteration since last report, further than that the steel may be moved more freely in every direction, except when pulled directly downwards, when it seems to be retained as forcibly as *at first*; the probe can be introduced into the wound, upwards and inwards, nearly four inches, and can with some difficulty be made to move round the steel; but no information as to its size and shape can be gained from this mode of examination. It occurred to me at this time that it was a hook, and that it might be retained by catching on one of the ribs. Having no pain except from the pulling, and being still averse to the use of the knife, the same treatment was pursued.

August 5. The foreign body having become very little loosened, and now causing more pain on its being moved, I made a deep incision of about three inches in length over its course upwards, using it as a direction, when it was easily extracted, and found to be a common kitchen fork, broken off close to its handle, and with one of its *two* prongs wanting about an inch of its point; it was blackened and in some degree rusted. It seemed to have been retained by a bridle of muscular fibres embracing its

shoulders, for it was immediately liberated when this part was divided by the knife.

The wound was dressed simply with straps, and healed so soon that in ten days the man was doing duty in the boats and aloft.

Strange as it may seem, even after its extraction, the man persisted in adhering to his original statement of his being ignorant, how and when it had been introduced; and during the two months I remained in the ship, I was not able to gain further information in the matter.

He seemed to have no defect of memory in any way, for he, without hesitation, gave me every information I asked as to his former life and habits. He is a native of Devonshire, has been at sea since he was twelve years of age, and in the merchant service till two years ago, when he joined his Majesty's ship *Tweed*, at the Isle of France, and from which ship he was paid off immediately before joining the *Belvidera* in February last.

I have thought it proper since my arrival in England in November, and before forwarding this case to you, to write to the patient's father, residing at Topsham, Devon, for information as to his early life; and I have received an answer from his mother, in the absence of her husband, in which she states that he received no wound or hurt while under the care of his parents, and that for the last eight years he has been wholly from home in various parts of the world in the English and American merchant services, during which time he makes no mention in his letters of having received any hurt; and she further adds, that she had not received any letter from her son mentioning any injury whilst out of England in the *Belvidera*.

Setting aside his own statement altogether, my own opinion is, that it must have been in his back for many *months*, if not for *years*, judging from the indistinct and ill defined mark left, *taking it for granted* that this was the wound by which it had been introduced, but which is yet problematical from the little pain he experienced from its presence; and more especially from the knowledge, that, during the previous *months* which he belonged to the *Belvidera*, he was never one day off his duty or on the sick list. Your readers are, however, as well able as myself now to form conjectures on the subject.

Having already experienced a difficulty in convincing some sceptical individuals of the facts above related, I may in justice to your readers and myself state, that as the case excited great interest, *while under treatment the patient was seen* by the honourable Captain Dundas, Dr. Tweeddale, and most of the officers and crew of the ship, and also by Mr. Donnett, Mr. Geddes, Mr. Chantres, surgeons, and Dr. Jones, assistant surgeon, Royal Navy; and the fork was extracted in the presence of Dr. Tweeddale, who assisted me, and Lieut. Ogden of the Royal Marines.

*** The annexed engraving is of the exact size of the fork. It is represented with the concave side upwards. The marks on the prongs (which have not the solid appearance of English manufacture) are those of rust. In all other respects the instrument is perfectly smooth, and has a bronzed appearance.

A, the part first seen. From A to B, the extent seen before the day of its being extracted. C the point of the broken prong, which is ragged, and seems to have been fractured by violence most likely before its introduction.

No rational person can for a moment suppose that the ignorance of the man was real. The wound caused by the introduction of such an instrument must have been in the highest degree severe, and an effort to withdraw it appears to have been the cause of the forcible fracture near the handle. The position of the fork was singular, as a stab is so much more likely to have been made downwards than upwards.—Ed. L.

WESTMINSTER MEDICAL SOCIETY.

Saturday, January 28th, 1832.

Mr. Chinnock in the Chair.

DR. PHILIP'S ADVERTISEMENT.

The subject of the *advertisement* was again brought forward this evening.

Mr. Chinnock announced that the committee, according to the resolution passed at the previous meeting, had taken this sub-

ject into consideration, had written to Dr. Philip on the question, and hoped soon to conduct the matter to a proper termination.

Dr. FERGUSON was desirous that, whatever might be the result, publicity might be given to it in those periodicals in which the advertisement had appeared.

Dr. SIGMOND spoke in exculpation of Dr. Philip himself from any intentional misrepresentation in the transaction, and was confident that doubts would be satisfactorily removed. Dr. Sigmond also alluded to the appearance that day in a medical journal, of what he designated an "*attack*" on Dr. Philip.

Here Mr. CHINNOCK obtained permission to leave the chair, which was taken *pro tempore* by Dr. SIGMOND, when some observations fell from

Dr. FERGUSON, who stated that in that day's No. of THE LANCET his treatment of the subject in question had been misreported. The report was not in unison with what he had said.

Mr. CHINNOCK, having been the gentleman immediately concerned in bringing Dr. Philip's paper before the Society, wished to take the blame on his own shoulders, and was desirous of taking the first opportunity of clearing up, as far as lay in his power, the mist that obscured the facts of the case. Here Mr. Chinnock entered into an explanation of the circumstances, from which it appeared that it was at the request of Mr. Chinnock, and with the knowledge of the Committee, that the paper was produced, and by consent of the Society read, at a former meeting. Mr. Chinnock repelled with warmth any idea that Dr. Philip had made use of the name of the Westminster Society as a "catch;" that gentleman had not the least interest in the pamphlet, which was given to the world solely with a scientific object; he was too rich to look for lucre in such a matter, and in any other case had too high a sense of honour to stoop to such means.

Dr. STEWART also acquitted Dr. Philip from any culpability in this matter. He thought the objectionable term must have had its origin in a mistake. Although the committee had approved of the paper, that was a very different thing from making a "request" to its author to draw it up.

Dr. FERGUSON disclaimed any motive in mooting this question, further than that of preventing the adoption of the precedent in any future instance.

Dr. EPPS, Dr. GREGORY, and others, took part in the discussion, which was rather lengthened. The general impression was, we believe that the best plan was to await the answer of Dr. Philip to the communication forwarded by the committee.

[Being obliged, from the nature of the subject, to have an eye to condensation, the foregoing only conveys the general *tenor* of the discourse. In our short statement given last week, we are ignorant of having in the least deviated from the truth, still more unconscious of any "attack."]

Mr. CHINNOCK having resumed the chair,

THE ANATOMY PETITION

was read on the motion of Dr. WIGHT.

The petition, which was drawn up by the sub-committee, ascribed the existing prejudices against dissection to two circumstances. 1st. The illegality of dissecting, or possessing for dissection any dead bodies but those of murderers (so we understood); and, 2dly, the consignment of the dead bodies of murderers to the knife. It prayed, therefore, for the abolition of this portion of the statute law, and also for the infliction of severe penalties on the *buyers* and *sellers* of bodies, as essential to the prevention of "burking." In conclusion, the system adopted in France was held up to the Legislature as an example which the petitioners begged to suggest for imitation in this country.

Allusion having been made to the objection advanced by one of the committee on a former evening to the adoption of this petition, it was observed in explanation that the members of the committee were not to be considered as having pledged themselves to the sentiments therein expressed.

Dr. WIGHT suggested an amendment expressive of the honourable practice of dissection. He moved that, in addition to the prayer for the abolition of that part of the statute which consigns the body of the murderer to dissection, they should also pray of the Legislature to pronounce the bodies of executed murderers *dishonoured*, and unworthy to be used for the benefit of society.

Dr. STEWART agreed in the spirit which dictated the resolution of Dr. White, and thought that medical men should not be "*the finishers of the law.*"

Dr. GREGORY would obtrude for a moment, in order to express his objections to parts of the petition. It appeared that the Society had one of two modes to pursue. To address Parliament, calling their attention to the subject generally—which, however, the present bill rendered unnecessary—or to recommend some specific measures. The Westminster Medical Society now indeed stood so high, that any recommendations coming from its numerous members, might be supposed well worthy the attention of the Legislature. Looking then at the proposed petition, he was struck, first, with the statement ascribing the prejudice against dissection mainly to

the circumstance that it formed a part of the sentence of the worst of criminals. Now he did not contend for the continuance of such a "punishment," but he thought that if abolished to-morrow, it would not abate one jot the prejudices now in existence, which were, in his opinion, co-existent with human nature itself. The next objectionable part of the petition was that respecting the buying and selling of dead bodies. Impressed as he was that after the passing of the act, the practice of exhumation *must* continue, he did not know how far it would be right to give a decided opinion that the *buying* and *selling* should be interdicted by severe penalties, and as for recommending the adoption of the French system, he was not conversant with its details, and consequently could not properly point it out as a desirable precedent. He would like to see abrogated all the penal laws in existence against dissection.

Mr. KING went into a review of the arguments of Dr. Gregory. He certainly thought that the penal statute against murderers might be considered at least *one* cause of prejudice. Was it indeed singular, that an objection should be entertained to submit to anything which could not be inflicted legally on any but murderers? In France it was considered by professional men quite beneath them to dissect any one who had been guillotined. The objection of the honourable gentleman to the punishment of buying or selling the dead body, was founded upon a basis not very stable. For the practice of exhumation would be unnecessary, if the Legislature would only allow to be brought into play those facilities to the study of anatomy which were offered in the usual course of things. (Here we understood Mr. King to allude to the number of unclaimed bodies as forming an abundant source of supply.) Dr. Wight's suggestion he considered fully accordant with what ought to be, but he questioned the propriety of making at once so long a stride—that that which had long been looked upon as a badge of disgrace, should all on a sudden be ordained a mark of honour. This would certainly "astonish the natives." (*A laugh.*)

Dr. WIGHT observed, that one of the popes finding the strength of prejudice against dissection in the Roman states, forbade the dissection of murderers. In a few years the objections had vanished.

Dr. EPPS agreed with Dr. Gregory that there existed a *natural* repugnance to dissection. There was an almost universal feeling of dislike against exposing to vulgar gaze the remains of a being who in life had shared our love or friendship. He might support his views by a reference to the Spartan women, who were once afflicted with some disease which proved so horrifying to their feelings, that it became a common practice to end their lives by suicide. To such an extent did this proceed, that it was determined that all who might be guilty of the act should be exposed after death publicly in the market place. This had the desired effect. Not one after this edict followed the example of self-destruction. He advocated discontinuance of the dissection of murderers, because, whilst it tended not to deter men from the commission of crime, it was frequently an unjust violence to the natural feelings of surviving relatives. The unclaimed bodies in London amounted, he understood, to 300 or 400 annually, and now that the restrictive by-law of the College of Surgeons had been expunged, there would be found little difficulty in obtaining a sufficient supply from such a source, as the country schools would draw off the crowd of students from the metropolis. He thought that a good effect might be produced by burying murderers in some separate spot, and as the monuments of great men were records of their virtues to incite us to follow their examples, so the father might point out to his sons for execration the desolate grave of the murderer.

Dr. SIGMOND thought that instead of pointing out to Parliament details for their adoption, much good might have been done by making some forcible appeal on the necessity of dissection, which was a point far from being generally allowed. It was, unfortunately, still necessary to prove to some men that we were not studying anatomy for our own pleasure.

Dr. GREGORY, alluding to the proposal for supplying the dissecting-rooms with the unclaimed bodies, asked what was to be done where, as in Edinburgh, there were seldom, if ever, any bodies left unclaimed?

Dr. FERGUSON thought, that in Edinburgh, where there are so many schools, it may be that those who undertook to supply them, in numberless instances claim the bodies of the poor as their relatives.

Dr. STEWART thought that nothing was more necessary than to prevent the buying and selling. No *money*, no *burkers*. He quoted from Dr. Somerville's pamphlet, in which it was related, that *before* the "Burk" murders, several medical men had supposed the existence of such practices, and warned the authorities against them. This was a sufficient answer to any accusations against the medical men. Had their suggestions been attended to, the loss of life might have been averted.

Mr. KING characterised the buying and selling of bodies as most disgraceful. Nothing on earth would prevent murder for

the sale of bodies, but making those bodies worthless in a pecuniary point of view.

After some further discussion, the petition was put to the vote, and adopted as the petition of the Westminster Medical Society. It is to lie on the table at the next meeting to afford members an opportunity of attaching their signatures.

ENORMOUS CONGENITAL UMBILICAL HERNIA.

At a late sitting of the *Société de Médecine Pratique*, M. Berthelot read a paper on a case of congenital umbilical hernia. About the end of last October, a woman, living on the Quai de l'Horloge du Palais, was delivered of a male child, having gone her full time. The infant had an umbilical hernia, ten inches and a half in its middle circumference, seven inches at its base, and three and a half in length. It was formed by the dilatation of the umbilicus, and was not covered by the skin, which terminated in a circle around the base of the tumour, as upon the umbilical chord. The sac, in which the ruptured parts were contained, consisted of the union of two serous membranes—externally that of the chord, internally by an expansion of the peritoneum. The chord had not its insertion on the apex of the hernia, but on the right side near the abdomen, and its vessels glided between the two leaves or coats of the cyst, for the space of an inch before penetrating the cavity. The sac was in colour pearl-white towards the left, on the right red and shining. One side of the external surface of the sac became inflamed a few days after birth, discharging a fœtid pus. Many of the internal organs being developed in the sac, the abdominal parietes had very little extent, so that the hernia permitted of no attempt at reduction, even supposing the contained parts to have been free and not adherent to the walls of the sac. M. Berthelot held out to the parents no hopes, but directed them to afford the infant the usual nourishment. A similar prognosis was given by many physicians who were consulted. The infant died in twenty days, having all that time, up to the moment of death, become more and more emaciated, although fed plentifully on milk.

The result of the post-mortem investigation was as follows:—A circular incision being made on the abdominal parietes outside the tumour, in order to be able to draw it entirely back, M. Berthelot found that the contents of the hernial sac were—1st. In its superior left part, the stomach. 2nd. On the right of that, the liver, excepting the lobulus spigelii. 3rd. Below the stomach, all the intestines except the descending colon and the rectum. Lastly, in the bottom of the sac the spleen, which was but slightly developed. The liver was not united with the diaphragm, but suspended by a considerable elongation of the hepatic vessels. Three quarters of its superior surface on the left side were completely adherent to the inner surface of the hernial sac. The remainder of its superior surface, and the whole of the inferior, were free and smooth. The part adherent was red, granulated, and appeared to have been the seat of recent inflammation, which was probably the cause of its adherence. The small intestines had also some points of adhesion to the internal walls of the cyst, but possessed none of the redness. The abdominal cavity contained no pus. The whole of the intestinal tube was healthy, except about an inch and a half, presenting, where it was adherent, an evident stricture, with thickening of its coats. During the twenty days of this infant's life, the whole functions were properly fulfilled.—*Lancette Française.*

LONDON HOSPITAL.

ALLEGED BREACH OF CONTRACT BY A LECTURER.

To the Editor of THE LANCET.

SIR,—I should not have troubled you again, if Mr. Ramsbotham had confined his "Reply" to a palliative excuse for Dr. Davies; but as he has endeavoured to cast an imputation upon the validity of my statement, I trust you will, as a matter of justice, insert the following explanation of what I there stated.

His attempted justification of Dr. Davies in my own case amounts to just nothing, inasmuch as he merely dates the "breach of contract" "several weeks ago," and on that account is silly enough to call it "no breach." The "Prospectus" told me that lectures would be given at such an hour, on such a subject, by such an individual; and on the faith of that representation, I make my arrangements accordingly, pay down my money, and enter the school as a perpetual pupil: *afterwards* an alteration is made (it matters not whether the next day or three months after) against my consent, as one of the contracting parties; and I would ask any one who possesses common sense, what he would call it but a "breach of contract?" I certainly *did not attend* Dr. Billing, being at that time engaged in the acquisition of anatomical and other preparatory knowledge; nor was I at all aware of the alteration till I saw the notice at the theatre door. I have now to compliment Mr. Ramsbotham

upon his ingenious, though unjustifiable, method of hunting out the naughty Victima; but I must tell him, that although he made extensive inquiry, it was neither universal nor effectual. What the "peculiar advantages" are which "this hospital affords" between three and four o'clock in the afternoon, I have yet to learn. But the humbug of this part of the "Reply" is truly ridiculous; for the truth of the matter is, that Dr. Davies's extensive practice will not allow him to lecture at that hour. The quotation with which I concluded my letter was intended to apply to hospitals collectively, and not in particular to the London Hospital. I am rather intimately acquainted with Dr. Davies, and although a silent and humble, I am an ardent, admirer of that gentleman's brilliant talents as a practitioner and teacher. The other lecturers I am not at present so well acquainted with, but I shall, as occasion may offer, "stir up their *pure* minds by way of remembrance."—I am, Sir, your obliged,

VICTIMA.

Jan. 30th, 1832.

BEQUEST OF A BODY TO THE

LONDON COLLEGE OF MEDICINE

FOR THE PURPOSES OF DISSECTION.

To the Editor of THE LANCET.

SIR,—I agree with you entirely respecting what should be the basis of an anatomy bill, as well as in your condemnation of the system pursued by the surgeons of the London hospitals in their post-mortem examinations, and am moreover of opinion that every person who has the welfare of society at heart, and is a friend to science and humanity, ought to bequeath his body for dissection; holding this opinion, I hereby authorise you to make known through the medium of your Journal, that I this day bequeath my mortal remains to the LONDON COLLEGE OF MEDICINE, to be dissected for the instruction of the students of that institution, whoever they may be; and after dissection the bones to be used for the purpose of teaching the rudiments of anatomy. My example will I trust be followed, *religentem esse oportet, religiosum nefas*. Nay, some have already led the way, both here and elsewhere. Surely it is a far more rational mode of thus disposing of our bodies for the benefit of the living, than to have them committed to the earth to be eaten by worms,—to me it seems more in accordance with the spirit of the Christian religion.

The examination of the upper eyelid of the left eye I beg may be conducted with great care, because in consequence of a fall which I had when nine years old, in crossing a corn-field which had just been reaped, a grain of corn was in some manner or other forced deeply into its inner surface near the external canthus, where it soon became encysted, and will, I have no doubt, be found in good preservation. It is quite perceptible to the touch, is not attended with the slightest inconvenience, and has retained the same form and size for the last thirty years.

It may be asked why I leave my body to a college which is only in embryo, and which has not even professors appointed to it yet. My reasons are these:—

First. Because, though possessing a feeble and broken-down constitution, I hope, barring accidents and the cholera, to live to see students flock in hosts to the LONDON COLLEGE OF MEDICINE.

Secondly. Because it is based on principles far more liberal and enlightened than those of any of the present existing medical institutions in Great Britain.

Thirdly. Because it requires from candidates for its honours actual proofs of the possession of a thorough knowledge of everything relating to the profession, and, unlike the institutions now in existence, wisely disregards the schools in which such candidates study, as well as the number of certificates they possess,—things which afford no evidence whatever of students' acquirements, and which some of the greatest blockheads possess in very great abundance.

I am, Sir,
Your very obedient servant,
A. COURTNEY, Surg. R. N.

Broadstairs, January 1st, 1831.

STATE OF THE MEDICAL PROFESSION.

To the Editor of THE LANCET.

SIR,—The study of my profession having cost upwards of 500*l.* ere I could designate myself a "general practitioner," independent of the extreme mental anxiety ever attendant on presenting for examination, I returned home in expectation of meeting with that success in practice which such an enormous expenditure of money demands. Shortly afterwards I commenced business, having several highly-respected surgeons to cope with, long established in the practice of their profession. To this class of practitioners I bow with extreme gratitude, and earnestly wish to continue the honourable struggle of obtaining, by fair means, any portion of public confidence. Gentlemen possessed of independent and honourable principles in the practising of their profession can never be considered as barriers, or stumbling blocks, to the well-

doing of newly-commencing practitioners. Ere long, however, I discovered in the person of a chemist and druggist (though assuming the garb of a regularly-educated man) an individual who, by mean artifices, stratagems, and reports, hath cleverly circulated in the town and neighbourhood that he is a "doctor"—good and bold. Believe me, Sir, he prescribes and mixes up more physic than any medical man in town; he hath long pursued this career. Two cases, for which he alone prescribed, terminated fatally the other week. Such events frequently take place. A highly-respected medical gentleman was called in to one of his late cases. On his arrival he found the patient, a young lady, sitting on a chair, with her arm and head reclining on a table, on which a pillow was placed. On his touching her arm he was shocked at feeling it cold and pulseless. Alas! she was dead—quite dead! The chemist had never visited her! She was in the last stage of fever, petechiæ had begun to make their appearance, and her daily allowance consisted of dry toast, gruel, and cold water.

I am acquainted with two or three other cases equally glaring in ignorance. Such instances are horrible to relate. To think that a beautiful young lady, who a very short period before was in the meridian of happiness, should thus be allowed to die, without one effort being made on the part of a regularly-educated medical man to snatch her from the jaws of death! How long must such practices be continued? They are of no rare occurrence in the country. Is there no law—no protection against such dreadful consequences? Are our diplomas only waste paper and our profession a bug-bear?

I am, Sir, yours truly,

W. H.

Jan. 1832.

CHARGE OF UNPROFESSIONAL CONDUCT AGAINST DR. R. CLANNY.

To the Editor of THE LANCET.

SIR,—I submit the following to your judgment:—

Doctor Clanny of Sunderland visited a patient of mine, with me, named Hannah Barnes, who was labouring under epidemic cholera. In the same house resided Elizabeth Taylor, who was unwell from a stomach complaint. Dr. Clanny requested permission to see her; I granted it. Immediately he saw her, he said her disease was epidemic cholera. I remonstrated, and informed him Doctor Brown had seen the patient, and agreed with me in opinion. Dr. Clanny departed, and about two hours after, I received the following note:—

"Sunday evening (8th Jan., 1832).

"My dear Sir,—I have been thinking much about the female patient whom I saw last, down stairs, and I cannot get over the impression that she is affected with epidemic cholera. Yours very truly,

(Signed) W. REID CLANNY."

Doctor Miller (who has been in Asia) was visiting some patients with me at the time I received the above; he went and saw Elizabeth Taylor, and declared that her complaint did not bear any resemblance to cholera. Doctor Clanny, when he found he was unable to induce me to report it as cholera, informed the Board of Health, Sunderland, that I was withholding cases that occurred in my practice. This I allowed to pass, as Doctor Miller, a member of the Board of Health, had attended all the cases with me. Shortly afterwards I was favoured with a note from the Board of Health, requesting my attendance to answer a charge laid against me by Dr. Clanny, member of their Board. I attended at the time requested, but to my disappointment Doctor Clanny was not present to substantiate his charge. I repeated to the gentlemen present what I had said to Doctor Clanny, and the conduct he had been pursuing. With the dignity and good feeling of gentlemen, they said I had been misrepresented, and that they were satisfied.

Now, Mr. Editor, was it proper or professional for Doctor Clanny to send a person to visit my private patients without my knowledge or consent; to compel me to neglect my professional duties by coming a considerable distance to the place appointed for explanation, and then not to make his appearance?

Mr. Editor, the late noble-minded and liberal Doctor Armstrong had to grapple with this Doctor Clanny. I would not have made an allusion to the much-lamented Dr. Armstrong, had it not been that the patient whom Dr. Clanny wished to designate as labouring under cholera, occupied the room in which Dr. A. was born, and where he first devoted his attention to the study of medicine. I am, Sir, your obedient servant,

WM. CANDLISH.

27th January, 1832.

Ayres Quay, near the Quarantine Station, Sunderland.

*** Prudential motives have induced us to omit two or three passages which were contained in the foregoing communication. The parts omitted were scarcely essential to establish the chief points of the narrative.—ED. L.

BLUE STAGE OF THE SPASMODIC CHOLERA.

Sketch of a Girl who died of Cholera, in Sunderland, November, 1831.

BLUE STAGE OF THE SPASMODIC CHOLERA.
Sketch of a Girl who died of Cholera, in Sunderland, November, 1831

THE LANCET.

London, Saturday, February 4, 1832.

HISTORY

OF THE PROGRESS OF THE MALIGNANT CHOLERA IN ENGLAND AND SCOTLAND.

Since the publication of the lengthened article on the general history of the cholera of India and Europe, with which we presented the readers of this journal on the 19th of November, 1831, we have anxiously looked for the opportunity of placing before them, a compendious statement of the additional knowledge of the disease acquired through its unfortunate prevalence in England, and the adjoining districts of North Britain. Facts of paramount practical interest preclude any further delay in the execution of this important object. Three months having barely elapsed since the arrival of the pestilence on our shores, it has already traversed a large surface of the country; it has braved the reduced temperature of a Northumberland winter; it has cut off one thousand victims, and attacked thrice that number of individuals, and it is now exercising its fatal virulence in the centre of the metropolis of Scotland.

The readers of the voluminous article alluded to will remember, that in its pages we carefully traced the successive irruptions of the cholera, from the Ganges to the Wear, marking, as we proceeded, the numerous appalling events which signalized the various stages of it progress. We drew from the labours of the Anglo-Russian Commissioners, a picture of the semeiology of the malady as they witnessed it in the North of Europe, and we derived from the researches of the Indian writers the physiological explanations which these symptoms received from the actual state of our knowledge regarding the normal functions of human life. Thirdly, we collected, from every source, almost innumerable data regarding the anatomical pathology of the disease. Fourthly, we entered minutely into the controversy regarding its mode of propagation, and showed, by insurmountable and multitudinous facts, that, upon every principle of reason, we are bound to consider human intercourse as the chief agent of its dissemination. In the fifth place, we gave an ample summary of every mode of practice employed in the treatment of the disorder previous to the period when we wrote, specifying the effects of each mode on the amount of mortality; and, lastly, we dwelt emphatically on the nature of the sanitary measures which were sanctioned by the deductions from the facts which the article recorded. On each of these divisions we now proceed to bestow a few remarks, embodying the information which the prevalence of cholera in Great Britain has added to our previous store.

IMPORTATION INTO ENGLAND.

It is not our intention to enter minutely in this place on the serious questions regarding the origin of the calamity in this country. Whatever be the truth, it still, we lament to say, remains enveloped in the clouds of local dispute, and grovelling subserviency to pecuniary interests, among many whose profession should have given them a different bias. Thus much is, however, satisfactorily ascertained, that the *quarantine regulations* in the port of Sunderland were, for weeks before the official announcement of the introduction of the malady, so loosely, so negligently, observed, that more than one practitioner of the town openly expressed his conviction that the cholera would there find its first and its easiest entrance. It is also a notorious fact that more than one case of cholera occurred before the publication of the official announcement; and we have, further, strong reason to believe that chests of clothes, be-

longing to persons deceased from cholera at ports infected in the Baltic, found their way into Sunderland so early as August last. The excellent private character and the known benevolence of some of the individuals whom any inquiry into the facts alleged might principally implicate, have generated a degree of reluctance to move upon the subject. It is, consequently, very probable that no detailed authentic information regarding it will ever be obtained. Balanced as the evidence is at present, we must, however, conclude that the *importation* into Sunderland of the *blue cholera* has not as yet been *proved*, but that it is rendered highly probable. Where is there, in the whole history of disease, an example of a malady travelling *without human aid* across a wide tract of sea, selecting one particular port for its entrance into a certain country; confining its early influence to a circle of ground not one hundred yards in diameter, and then creeping on slowly in various directions? The occurrence is unprecedented in the chronology of disease, and impossible according to the recognised physical attributes of the air we breathe, and of the terrestrial surfaces which we inhabit.

We shall not dwell at any length on the disgraceful examples of the subornation of medical opinion, and the recklessness and avarice of a knot of mercantile speculators, exhibited during the early periods of the Sunderland epidemic. What now must be the feelings of the men—convicted either of gross and unexampled ignorance, or of equally shameful venality — who, with bared faces, erect and conspicuous before the world, continued to deny the existence of an unusual malady, until the choked grave-yards of their town bore witness to the deplorable fact! What, we repeat, must be their feelings now, when, as the *possible* results of their obstinacy or knavery, one thousand of their fellow-creatures have, within three months, fallen victims to the pestilence! But enough of this: we should not have touched at all upon this branch of the harassing subject, had we not felt it our solemn duty, as annotators of events and caterers for the future historian and observer of the disease, to hold up these occurrences for their reprobation and guidance, and show them, that if some portion of the medical press of the day permitted the circumstance to escape without the severest censure, the whole was not equally silent.

PROGRESS IN SUNDERLAND.

Of the earlier events and progress of the cholera in Sunderland after its introduction, a pamphlet* now before us affords an instructive picture. We condense here the author's account of the original cases of the disease, separating them from all collateral circumstances respecting mode of propagation, treatment, &c.

"The following information relative to the first appearance of what by many was considered to be the Asiatic cholera, in Sunderland and its neighbourhood, was communicated to me by Dr. Ogden, one of the inspectors appointed by the Board of Health of Sunderland, to examine and verify the cases of disease given in the daily official reports. Early in the morning of August the 8th, 1831, a man named Arnott, æt. 40, was attacked by a disease closely resembling cholera, and he died at the end of twelve hours. He was a quarryman, in decent circumstances, and resided at Pallion, on the river Wear, about two miles from Sunderland. Two days afterwards a potter named Allison, living at Southwick, on the north side of the river, suffered severely from a disease similar to that which destroyed Arnott, with whom he was not connected; Allison recovered. On the 14th of August, Robert Henry, of Fisher's-row, Sunderland, died after an illness of thirty hours, his symptoms during the illness resembling those of cholera.

"Another case, similar to the former ones in its character, occurred in Bishop-Wearmouth early in October; this did not terminate fatally; and from this time no other case was noticed as being in any degree suspicious, until the 26th of October, when the first death from acknowledged cholera took place. This was the case of the elder Sproat, who was a keelman, re-

* Observations on Cholera, made during a visit to Sunderland, undertaken by direction of the Birmingham T. I. Board of Health, in the months of Nov. and Dec., 1831. By George Parsons, Surgeon, &c. 8vo. pp. 63. London: Longman. 1832.

siding in Long Bank, in the way to the Pier: he had had slight diarrhœa several days before his death. During his illness this man was seen by Mr. Kell, a regimental surgeon then at Sunderland, who at once pronounced the case to be one of Indian cholera, which disease he had seen in the Mauritius. On the 27th the son and grand-daughter of Sproat, being then ill with cholera, were removed to the infirmary in Bishop-Wearmouth; they both appeared convalescent on the following day, and the girl recovered; but on Monday, the 31st, the son had a relapse, and died after a few hours' illness. On the same day, the 31st, two other men died of cholera; they resided in different parts of the town, were unknown to each other, and were supposed to be personally unacquainted with the Sproat family. One of these two men was a keelman named Wilson, he was 45 years of age, and resided in the third story of a house in the High-street, Sunderland; he died after 14 hours' illness. The other person was a shoemaker named Rottenby or Rottenburg, he lived in a low and confined house on the Monk-Wearmouth shore.

"On the 1st of November, a nurse at the infirmary in Bishop-Wearmouth, who had assisted in removing the dead body of the younger Sproat from the ward of the hospital, but who had not attended him during his illness, was attacked by the disease, and died at the end of six hours.

"No other cases of cholera were reported from the 1st to the 6th of the month; but on the 6th, 7th, and 8th, cases of the disease again appeared in different parts of the town. On the 6th, a Mrs. Wilson, living in Nile-street, an old and intemperate female, was attacked by the disease, and died after 16 hours' illness. On the same day, a pilot named Ellemore, living in Silver-street, died of cholera after 21 hours' illness; and on these three days other cases were observed in Warren-street, Silver-street, Burleigh-street, Golden-alley, Robinson's-lane, Harris-lane, Fitter's-row, and Low-street; and to these, and the adjoining streets, Mill-street, Sailor's-alley, Baine's-lane, &c., the disease in Sunderland parish was in a great degree confined.

"By the 12th of December, according to the official reports of the Board of Health, there had been since the 26th of October, 152 deaths from cholera in the whole of Sunderland, and of these at least three-fourths had occurred in the parish of Sunderland, which contains only one-half of the whole population."

From this essay, and other publications on the subject, it appears, that during the entire Sunderland epidemic, the total amount of seizures and mortality up to the 26th January, was 536 cases, and 202 deaths. With respect to the medical topography of the town, no good account has yet been published. The population may, however, be correctly computed at 40,000 persons. During the epidemic, the maximum number of cases did not exceed 30 daily, and that for a very limited period. The malady attacked comparatively few individuals in comfortable circumstances, and was most fatal among females, old, or very young, persons, and those previously infirm, or of intemperate habits. Altogether, the period was one of unusual excitement in the town, owing to the crowds of medical men, some of great celebrity, attracted from all quarters of Great Britain, and from the continent of Europe. Generally speaking, every disposition existed on the part of the respectable practitioners of the town to afford facilities for inquiry to the numerous vistors. One surgeon, especially, Mr. Embleton, senior, in whose practice a great majority of the cases occurred, is unanimously spoken of as deserving the gratitude of the profession, for his ready and inexhaustible kindness. We are at the same time bound to say, that we have good reason to believe that other practitioners in the district, were far from exhibiting an approach to Mr. Embleton's liberality; and in order to secure a monopoly of the press, withheld many of their cases, and concealed the results of their researches, both from the professional and the public eye. We should add, that the official charge of the districts was consecutively confided to Dr. Daun and Dr. Gibson, whose conduct and demeanour are highly commended by those who were cognizant of their exertions.

No meteorological phenomenon of the least importance marked the prevalence of the cholera in Sunderland; or, indeed, we might add, has characterised its subsequent extension,—the part of its history to which we shall next advert. It was not until the second week in December that the disease seemed disposed to abandon its haunts in Sunderland, and seek a wider field for the exertions of its destructive energy. The villages around Sunderland began to be affected, and the fishing port of Seaham, North Shields, Houghton-le-Spring, Chester-le-street, Blyth, and several other

places, began to suffer. The precise dates of the irruptions in these districts have not been authentically published, or, indeed, we believe, at all ascertained.

NEWCASTLE, GATESHEAD, AND THEIR NEIGHBOURHOOD.

The towns of Newcastle and Gateshead now commenced to attract the attention of the public. Some days after the earliest occurrence of cholera in Sunderland, a man named Oswald Reay died, under symptoms which, though doubtful at the time, can no longer be mistaken as denoting the disease in its worst form. Dropping cases now succeeded throughout the town; a man named Jordan died in a few hours, and his case occasioned much controversy, the merchants, as usual, exclaiming it was not the disease. The sensible, experienced, members of the medical profession too clearly perceived the truth. Dr. MACWHIRTER and others declared their conviction of the identity of the diseases; but all doubt was soon destined to be at an end, and on the 11th of December a daily official report was first issued.

In Newcastle, as in Sunderland, the disease was at first confined to the banks of the river, and accordingly we find that Sandgate-street, a long and narrow winding street along the left bank of the Tyne, was, with the lanes running from it, chiefly infested. It is perfectly unnecessary for us to dwell minutely on the events which marked the prevalence of the malady in this place, so closely analogous were they to those observed in Sunderland. The population of Newcastle is estimated at 70,000, the total of cases up to the 30th January, 888; total deaths, 284. The observations respecting the class of persons seized in Sunderland, apply equally to Newcastle; and up to the 25th December the usual rate of seizures had been from one to fifteen daily. But at this time one of those appalling events so frequent in the history of this malady, involved the population of Newcastle and its suburbs in despair, and spread alarm over the entire kingdom. We allude to the remarkable irruption at Gateshead, respecting which we extract the following notice, from another clever pamphlet, by a gentleman of long experience in the malady, then a visitor at Newcastle.*

"Gateshead lies on the right bank of the Tyne, opposite to Newcastle, with which it is connected by a bridge. The population of the town of Gateshead is probably 10,000, and of the whole parish 15,000. The communication between the towns is necessarily very great. Up to the 25th December only two cases of cholera had occurred at Gateshead, although the disease had prevailed at Newcastle for eighteen days. On the evening of the 25th December cases began to be frequent, and by ten o'clock on Monday morning, forty had been seized, and ten had died. At ten o'clock on the 27th (Tuesday), there had been ninety-nine cases and forty-two deaths from the Sunday evening. I visited patients in Gateshead-fell, three miles from the town, on Tuesday morning. In one small village there had been ten cases and five deaths."

In Gateshead, up to the 30th of January, there had been attacked 391 persons; died 141.

Around Gateshead and Newcastle, the villages, especially to the North, now suffered severely. In some of the colliery hamlets, such as Walker and Seg-hill, the disease was especially malignant. In Newburn, a place of trifling extent, about five miles from Newcastle, the disease appeared in the second week of January, and up to the 29th of January, 292 cases have taken place, and 55 deaths. In this irruption, the rector and some other persons of comparative opulence fell its ready victims.

The movements and ravages of the cholera in the adjacent villages scarcely present any feature worthy of express contemplation. The district of Houghton-le-Spring, however, forms a remarkable exception, for here, according to the daily reports, and the correspondence of Dr. MACANN and Messrs. BULLEN and CATON with the Central Board, out of a population of about 2,000 persons (principally in the township of Hetton, population 999), there were 333 cases, and 70 deaths. The inhabitants of Houghton-le-Spring are in remarkably comfortable circumstances, and, on the average, conspicuous for their regular, industrious, and steady habits.

In North Shields, at the mouth of the

* Essay on Cholera, &c. as observed in India, Sunderland, Gateshead, Newcastle. By JAMES ADAIR LAWRIE, M.D. 8vo. pp. 74. Longman, &c. London, 1832.

Tyne, the cholera prevailed from a very early period of its arrival in England, but the seizures were very few, amounting only to 144 cases (43 deaths) out of a population of about 25,000 persons.

Such is a rapid view of the progress and ravages of cholera in England, up to the period at which we write. Besides the cases we have mentioned, some straggling examples of the malady have occurred: namely, one at Doncaster, two at Howick, two at Morpeth, and two at Durham. Several cases, besides, took place on board the ships in quarantine in *Stangate Creek*, and St. Margaret's Hope, and others in vessels at sea, and in the offings of Hull, Lowestoff, and of other ports on the coast between Dundee and the Medway. The precise number of these cases we have not ascertained.

SCOTLAND.

Forsaking now the continuous routes and creeping pace which had characterised its progress during the early weeks of its existence in this country, we find the disease bounding over an interval of nearly one hundred miles, and appearing suddenly in Scotland, in the town of Haddington, situated in the North Tyne, 16 miles south of Edinburgh. The first case in this town occurred on the 17th of December; the man attacked resided in a filthy close, by the river side; he was a man of dissipated habits, and died on the 20th December. This case was followed immediately by several others in the same locality, all on the river side. The population of Haddington is 5,600 persons. For the first ten days there were but 56 cases; on the 28th and 29th, however, a sudden exacerbation of the epidemic took place, and on the latter day several very respectable persons were attacked and perished; of these we may particularize the Procurator Fiscal of the county and an eminent schoolmaster of the town, both of whom died after a few hours' illness. The latest accounts up to the time when we write, give the cases at Haddington as 75, and the deaths as 41.

The village of Tranent, in Haddingtonshire, shows us the next irruption of the disorder. By the official correspondence, published in the 2nd number of the *Cholera Gazette*, it appears that on the morning of the 15th of Jan., a boy died of cholera in the village of Tranent, after an illness of a few hours; the same day the sister, æt. 25, of the first patient, died. On further inquiry it was found that an old woman had died under similar symptoms on the 11th, and her son, æt. 40, was seized on the 15th, and died next day. On the 16th the father of the boy and of the young woman, Nos. 1 and 2, was also attacked; after this the disease spread extensively, affecting chiefly beggars and intemperate persons, and in many instances attacking, in succession, members of the same family, or inhabitants of the same house. Tranent is about seven miles west from Haddington, on the great post-road to Edinburgh; it contains about 1,700 inhabitants, chiefly composed of colliers and other labouring people. Up to the accounts received this day (2nd February) there had occurred 106 cases and 41 deaths.

Tranent and Preston Pans are contiguous parishes; the latter is celebrated in history, and in the Waverley Novels, as the scene of the murderous conflict between the forces of Charles Stuart and the Southron army. On the 20th of January the disease showed itself here, three cases having taken place at its western extremity. The first case was that of a collier, æt. 30; the second his child, aged 2. The following day the coffin-maker who made the coffins for Nos. 1 and 2, was attacked, as were also two children of a widow who assisted the family of the first cases during their distress.

The districts of Tranent and Preston Pans lie close upon the south, or Edinburgh shore, of the Frith of Forth, and form part of a chain of villages situated at short stages between the capital of Scotland and the south headland of the Forth. Advancing to Edinburgh, at a distance of about 3 miles, Musselburgh, Portobello, and Leith, occur in succession at short intervals towards the capital. The first of these places must now engage our attention.

The village of Musselburgh forms one of the numerous fishing districts so plenteously scattered over the shores of Haddington, Lothian, and Fife, and is inhabited chiefly by a singular class of people, fishers exclusively by trade, and the reputed progeny of Dutch colonists on the Scottish coast. They intermarry only amongst themselves, have their own minister of religion, and preserve a variety of primitive and singular customs.

The females still maintain the picturesque and curious costume of the Flemish. They constitute, as we have observed, the chief part of the population of Musselburgh, which is composed of one long, wide, and straight street, of perhaps 300 houses. The utmost degree of friendly intercourse exists among all the inhabitants, so that the village may be, without exaggeration, regarded as one domestic establishment, and the villagers as members of the same family.

According to the official reports, the first case of cholera occurred on Wednesday, the 18th of January. It was followed next day by nine other cases, three of which died in the course of the day; next day, at 3 P. M., seventeen fresh cases were reported, and six deaths. From this time the disease continued to rage with a severity only paralleled in this country by the irruption at Gateshead, and by the reports up to the 2d of February had attained the frightful height of 219 cases and 93 deaths, out of a population of about 1,500 persons; so rapid was the mortality, that a pit was necessarily dug for the general reception of the dead, and the grave-digger became one of its earliest tenants.

Having approached thus closely to the metropolis of the North, the appearance of the malady therein was hourly expected, and accordingly we find, that on Friday the 27th of January, a man who went to Musselburgh to visit a sick relation, was attacked in the West Bow. Another case occurred next day in a woman, in Anderson's-close, same street, and a third in Skinner's-close, in the High-street. The disease had not extended much farther when the last reports were received. The authorities have issued directions for the closure of the theatres, and suspension of evening service in the churches; and according to a private letter received this evening, by an eminent mercantile house in the city, the High School has been closed, Herriot's and Watson's Hospitals have been placed in a state of seclusion, and the University shut up for a time. The bedding and apparel of the first patients were destroyed, *and the families removed* WITH THEIR OWN CONSENT *to* Queensbury-house, a spacious building, once a ducal residence, and some years since converted into an asylum for patients labouring under fever. To this admirable step we shall advert at greater length, under the head of Sanitary Measures.

The narration of another remarkable fact shall close our itinerary of the English and Scottish visitation of this awful disease. On the 22nd of January the disease broke out at Kirkintilloch, a small but opulent village within seven miles *of Glasgow*, and situated on the Grand Junction canal, which runs from the Frith of Forth to the Clyde, crossing the kingdom from East to West, and receiving tributary branches from Musselburgh, Tranent, and numerous other towns and villages around the metropolis. The population amounts but to 2,000 persons, yet by the accounts received on the 31st January, 37 cases had occurred, 12 of which had proved fatal. The utmost alarm was felt in the great city of Glasgow, the gigantic commerce of which, it is feared, must be considerably cramped by the near approach of the disorder.

MODE OF EXTENSION.

We have thus given a rapid but sufficient sketch of the *successive* irruptions of the cholera, not dwelling for a moment on the question regarding the manner of its extension. To this we now proceed succinctly to advert; but we must preface our remarks by a swift recapitulation of the statistical facts connected with its progress before its arrival in England, which we recorded in our first article on the subject.

In our article of November 19, we investigated the manner of dissemination of this malady by two separate modes of reasoning, the most conclusive which logicians can command. We *first* showed that a travelling disease, or one which appears over enormous surfaces of the earth in a succession of periods from a certain point, can only be propagated,

1st, By a vitiated or imperfect state of the air.

2dly, By exhalations from the earth.

3dly, By variations in the electric attributes of the earth or atmosphere.

4thly, By variations in the physical attributes of the air, such as its degree of heat or cold, moisture or dryness, &c.

5thly, By human communication occurring under various modifications; and,

6thly and lastly, By some unknown cause.

We next proceeded to examine each of

these causes in detail. Inquiring into the first, we found that by the geographical and chronological history of the epidemic, it was inadequate to account for its dissemination. We saw that it proceeded over numerous territories in direct opposition to the monsoon, and when accompanied by the monsoon, that its march was not accelerated. There are many other collateral reasons to strengthen this evidence, which we have not space to recapitulate here.

Neither was the second of the possible causes less opposed by irresistible, geographical, established, facts; one of which alone we shall adduce, namely, that while the surface of the earth and water was "hermetically sealed" by the ice of a Moscow winter, cholera raged therein with its most destructive power. There were also cited multitudes of additional facts, for which we must refer to our previous paper.

That variations of the electricity of the air or earth, were the agents in spreading the disease, was shown to be untrue, by the equal fury with which the cholera prevailed under diametrically opposite conditions of electricity of both these bodies. The same remark applies to the influence of astronomical changes.

Variations of heat and cold, moisture and dryness, altitude or lowness of level, were proved to be insufficient when we saw in the history of the disease that the virulence of the cholera was equal in the Indian Ocean and Siberia; in the deltas of the Ganges, Nile, and Volga, and over the limestone scalps of Tartary and the sands of the Arabian deserts; on the summit of Catmandou, the Caucasus and Carpathians, and in the marshes of the Sunderbunds, and Mesopotamian meadows.

Again; that intemperance of habit, or national customs, or deficient or depraved food, had nothing to do with the expedition of the malady from place to place, was proved by the contemplation of the numerous countries it devastated; the inhabitants of some of which observe the strictest sobriety, the customs in most of which totally differ; and by the fact that the most plenteous harvest, or the pinching failure of the crops, were alike deprived of influence on its progress.

What causes, then, remain unsifted? Two; namely, human intercourse, and an unknown power, only recognisable by its effects. To determine to which of these we were to adhere, we then inquired whether there were any direct facts on which the first proposition could be established, and we found, and collected, a body of direct evidence in its support, which, to any rational being, was sufficient to convey the most steady and permanent conviction.

A moment's consideration of the preceding statements is sufficient to show that we cannot reason correctly on this subject, if we limit our observations to one country or one set of people. We must take an extended view, embracing all the territories it has affected, and carefully noticing the opposite conditions of external circumstances under which it has prevailed. Heat or cold, then, dryness or moisture, the state of the wind, malarious exhalations, or a depraved state of the air, cannot have been the agents in question in this country; for the disease obeys but one remote cause, and all these have been shown by voluminous statistical facts to be unconnected with its progress. We are therefore left to decide between human agency, or the operation of an unknown cause. We proceed briefly to examine to which we may more rationally yield, according to the evidence this country has afforded, leaving out of consideration the direct testimony supplied by other nations.

In the first place, it is important to recollect, that owing to the total absence of any system of medical police in Great Britain, the tracing of any epidemic must depend upon chance alone; upon the casual diligence and scientific zeal of individuals, generally speaking too much occupied with practical pursuits to devote to such inquiries the time they invariably require. Again, in all diseases, no matter how virulent their contagion, or how universally acknowledged its existence, it has ever been a matter of the utmost difficulty to demonstrate unbroken concatenation in their irruptions. The histories of plague, typhus fever, and smallpox, fully illustrate and sustain this position.

For the proof of the influence of human communication in these diseases, we are for the most part contented to have recourse to a few remarkable and incontestable indivi-

dual examples. Having obtained these, we are afforded a clue to escape from the intricacies of an epidemic, which is snatching off its victims on every side; and we are satisfied to attribute the occurrence of many obscure and untraced cases to the imperfection of our means of investigation. One remote cause having been satisfactorily shown, the principles of reason, combined with the evidence of pathology, forbid us to seek another, and require us to refer anomalies to the agency of the multiplied collateral circumstances which either augment or diminish the spreading of a disease and obscure its mode of dissemination.

If, then, in the history of the epidemic we have just traced, we can show a few examples of the occurrence of facts evidently traceable to human intercommunication, we shall rest satisfied with the irrefragable proof thus afforded, that cholera *can* spread by contagion; and we shall patiently wait until chance, or an improved system of investigation, shall enable us to unravel the anomalies which it presents, and which we may be now unable to explain.

In all the towns and villages infected in Great Britain, members of the same families or connexions, though residing in *different houses*, have been the peculiar victims of the epidemic, the disease not appearing simultaneously, but in succession, in the persons affected. The state of our columns does not permit us to quote more than one example of these occurrences, but the daily journals are crowded with their recital.

Case 1.—Sunderland, Nov. 21.—"W. Knell, aged 30, a pilot, *living in Mill-street*. On the evening of November 20, he returned to his home, after having been out at sea; his clothes were then very wet, and he had fasted long. At nine o'clock in the morning of the 21st, he was out of his house; and soon after his return home he was attacked with cholera. When seen about mid-day, his pulse was almost imperceptible, his hands and face blue, respiration difficult, countenance indicative of much depression, and eyes sunk; he had cramps, but the vomiting and purging with which the attack began had ceased. At two o'clock P.M., he was removed to the Cholera Hospital, where he died tranquilly in the evening. *At the commencement of his illness he was attended by his mother, Mrs. Knell, who resided sometimes with him, sometimes with her daughter, Mrs. Elliot, of Warren-street.*

Case 2.—Nov. 24. "Isabella Elliot, aged five years, a niece of the pilot Knell, *residing in Warren-street*, was taken ill on Thursday morning (the 24th) with spasms, vomiting, and purging. She was seen at ten o'clock A.M.; her extremities were then very cold, the countenance blue, eyes deeply sunk, and vomiting of a fluid like rice-water continued. Four o'clock P.M., breathing difficult; temperature of the extremities and of the trunk increased; pain at the stomach; countenance still blue, but the expression intelligent; no cramps. Half-past ten P.M., the circulation restored; colour of the countenance improving; skin warm.

25th, at noon.—"Skin warm; no vomiting; purging continued; countenance lively and expressive, but the eyes still sunk; other symptoms favourable. Nine o'clock P.M., eyes more lively; face flushed, surface hot; respiration free; pulse rapid; is restless and feverish.

27th, ten o'clock A.M.—"Pulse quick; is restless; purging continues, conjunctiva injected. Seven o'clock P.M., breathing difficult; skin hot; spasms of the abdominal muscles, conjunctiva becoming opaque; lips dry; gums covered with black sordes. Died at four o'clock in the morning of Monday, the 28th.

Case 3.—Nov. 28. "Thomas Elliot, aged 34 years, a pilot, *living in Warren-street*, the father of Isabella E. At two o'clock on Monday morning (the 28th) he was visited by Mr. Torbock and Mr. Ainsworth. Elliot was then in a state of general collapse, eyes sunk, and turned upwards, features shrunk, lips dry, breath cold, feeble voice, no pain in the chest, stomach, or bowels; occasional purging, under which he almost expired; violent cramps in the legs; hands cold, and fingers shrunk in size; no pulse. Ten o'clock A.M., pulse now perceptible, cramps, vomiting, and purging, ceased; extremities and surface of the body warm; countenance still collapsed; breathing difficult. Two o'clock P.M., pulse increasing in rapidity; some fever.

29th, ten o'clock A.M.—"The feverish state has increased in intensity, has pain in the head, and is restless.

30th.—"The fever has assumed the form of bad typhus; has passed no urine. Aperient medicine given to him.

December 4.—"This patient gradually became worse, the delirium continuing, and the conjunctiva becoming much injected, but at no period was he unmanageable. On Friday night he sank into a state of coma, from which he was with difficulty roused. Elliot, his wife, and four children, lived in a single room, an attic; both the parents slept with Isabella E. during her illness. On the 26th of November, the father was

out at sea, and returned home very wet. On the 27th, he did not complain of illness, but was seized with cholera in the following night.

"Case 4. Dec. 7.—Mrs. Knell, aged 66 years, residing now with her daughter, Mrs. Elliot, *in Warren-street*. She had been slightly ailing for the last week. At eight o'clock last night she was attacked with vomiting, purging, and cramps; the vomiting soon ceased, and there had been no purging since six o'clock this morning. Twelve o'clock; her hands, legs, and body, generally cold; the nails blue, voice hoarse and feeble, countenance pale and collapsed, eyes sunk, no pulse; she lies quite motionless, but is sensible, speaking a little, but only when addressed. The temperature of her room was 77°, of her hand 70°, of her mouth 86°. Brandy and ether were given to her, and heat applied to the body. No re-action took place, and she died at eight P. M., remaining sensible to the last minute.

"Case 5. Dec. 10.—Alice Stafford, aged seven years, *lives in Fisher's-row*, and is a niece of the late pilots, Elliot and Knell. She was attacked with cholera last night at eleven o'clock, having been previously in good health. The surface of her body very soon became cold, and the fingers and nails of an indigo colour. During the night she had vomiting and purging of white fluids, like thin gruel. No re-action took place. Between nine and ten A.M. of the 10th she became insensible, and died at twelve.

"In addition to the five cases now detailed, another relative of this family fell a victim to the pestilence; the person was an uncle of the child Stafford; *he lived in Deaning-street, Bishop-Wearmouth*; was seized with cholera on the 8th of November, and died after a few hours' illness."

In the preceding cases we have a chain of communication traced between the members of the same family residing in *three different places*.

Again: the rate of mortality has been, in this country, greatest in the villages and hamlets wherein the inhabitants, though scattered over an extensive surface of ground, and residing in different houses, have lived on the closest intimacy, generally induced by equality of rank and identity of pursuits. Thus, in *Newburn*, out of a population of 5,000 persons, there were 296 cases and 56 deaths. In Houghton-le-Spring, of a population of 2,000, there have been 339 cases, 71 deaths. In Tranent and Prestonpans, of a population of 2,700 there have been 164 cases and 48 deaths; and in Musselburgh, 164 cases and 68 deaths. Hence it appears that the amount of seizures and mortality was, when compared with other places, in the inverse ratio to the amount of population, and in direct ratio to the extent of intercourse between the parties affected.

In the next place, we find that the individuals who by rank and pursuits were shielded from intercourse with the persons infected, did not themselves suffer from the disease in the large towns. On the other hand, and we state this fact with emphasis, the medical men, with few exceptions,—all indeed with whom we have met,—suffered from a symptom which is now known to be preliminary to this disease in a vast majority of cases, but which is perfectly amenable to therapeutic measures—we mean the diarrhœa—which of course attracted the immediate notice of the gentlemen alluded to, and was arrested by timely attention.

Lastly; we find, in nine-tenths of the towns and villages infected, that the disease commenced with one, two, or three cases, and then gradually rose to its maximum extent.

Such are a few examples of the facts which lead us to believe that the disease *can* be communicated by human intercourse, and such is the evidence on which the communicability of typhus fever, measles, and scarlatina, is admitted. But cholera, like these diseases, presents of course anomalies in its progress not invalidating the question of their contagious nature, but seeming to prove that infection is not the sole agent of their extension. We feel, however, not the least doubt but that these anomalies are principally referable to the influence of inexplicable collateral circumstances, on the "period of incubation," or "latent period" which elapses between the receipt of the first cause of the malady and the development of its symptoms—an event which, as is shown by indisputable facts, may be accelerated or postponed by an infinite variety of external agents, such as cold, heat, the state of the air, regularity or irregularity of habits, the state of the mind, &c., &c.

From this view, it appears to us, we can derive a more satisfactory explanation of the irruption at Gateshead—on the 26th of December, a day on which the populace indulge in every variety of dissipation—than

we can by referring that event to the operation of an unknown cause. Indeed, if we reflect on the number of the persons then attacked, who in all probability were previously in the latent stage of the malady, and the termination of which was doubtless accelerated by the dissipation of the day; and if we consider the number of persons predisposed by the same influence to the receipt of the morbific cause, and the almost instant development of its effects, we cannot be astonished that the numbers were so great and the mortality so excessive.

The sudden appearance of the cholera at Haddington, in North Britain, is adduced by a recent writer as a proof that it was not conveyed by human intercourse. But it is manifest that nothing more is proved by that event, than what takes place in *all acknowledged contagious* diseases, viz., that the communication has not been traced. We challenge the author in question to show us a solitary example of an epidemic travelling like the cholera, and one not acknowledged to be contagious, appearing, thus unexpectedly, at a distant point from the nearest scene of its operations, and then spreading slowly in the newly-visited district. No *known* cause but human intercourse could produce such an effect; and as to the operation of the "ignotum quid," we should not deem ourselves justified in admitting this, in the face of the conclusive facts to which we have already directed attention. Besides, it must be remembered, that all the reasonings of the author alluded to, are connected with his baseless notion, that little or no latent period exists in this disease. Dr. CRUIKSHANK, who visited the first case in Haddington, could not trace it to communication. Is the conclusion therefore legitimate, that no communication existed? Let us remember the constant intercourse between the town and the infected districts before we decide upon the question, and let us ask ourselves—Would such evidence as Mr. BELL's views regarding the first Haddington case, be received in a court of justice, or in any practical inquiry, as conclusive of the affirmative or negative of any proposition?

The other objections to the view we profess to entertain on this question, may be readily described and discussed. It has excited some astonishment, that if the malady was communicable by contagion, it should have dwelt so long in Sunderland without extending its ravages elsewhere. How little this fact bears against the contagiousness of the disease may be gathered from the circumstance, that the great plague of London, which commenced in Long Acre, spent five weeks in that street before it extended itself to the city, and that it desolated the latter before it reached Shadwell and Ratcliffe. So remarkably exempt, indeed, were the inhabitants of the latter parish, that numbers of wealthy individuals migrated thither from the west end of London, imagining that the plague would pass it by, and that the odour of tar from the vessels in the neighbourhood afforded a certain preservative.

But the progress of the cholera towards the North, and its slight extension in other directions, are also adduced as evidence against the hypothesis of contagion. Some have even gone so far as to endow the essence of the cholera with a degree of obedience to magnetic attraction! Had the builders of this vain hypothesis turned their eyes to the geographical history of this disease, they would have seen that every point of the compass has, at some time or other, seemed the special object of its journey; that while it was ascending to the North of the Himalayas, and westward to the Persian Gulf, it broke forth in the Mauritius, and, along the Indian Archipelago, to the South and East; that it ascended the Volga, and then descended to its parallel the Don; in fine, that it wandered in every direction where it found the most suitable materials, obeying the collateral, though secret, influence of circumstances, which render anomalous and perplexing the progress of all confessedly contagious diseases.

The last objection to which we will reply is a brief one, and will be as shortly noticed. Why has the malady, if contagious, died away at Sunderland and Gateshead without annihilating the whole population? We shall account for this circumstance when we are informed why the plague *ceases* in the Levant on the vigil of the feast of St. John.

We have now completed our notice of

this branch of the inquiry, satisfied that the principal obscurity which remains to be dispelled, relates to our want of knowledge of the true nature of the collateral circumstances which influence the spreading, or the cessation, of contagious diseases. It is devoutly to be hoped that our knowledge of this department of the subject will receive ample elucidation from the labours of the medical gentlemen who are now engaged in its investigation.

We have to lament, that the published accounts do not enable us to append to the preceding itinerary, a view of the various circumstances essential to the completion of the medical statistics of the disease. All the general accounts, however, agree, that the mortality has, in this country, been considerably greater among females than males. Since its commencement, the deaths have been, almost without variation, in the proportion of one to three of those attacked. Persons in opulent circumstances are reported to have been generally exempt, and intemperate individuals particularly affected. We must observe, however, that before these general statements can be received as elucidatory of the history of the cholera, attention should be paid to the small proportion which the numbers of the opulent bear to those of the poorer classes in the infected districts, and also the great numbers of those who habitually indulge in the abuse of spirituous liquors.

SYMPTOMS OF THE CHOLERA, AS IT HAS APPEARED IN ENGLAND.

The next department of the subject to which we have to allude, embraces the symptoms of the disease; and here it gives us pleasure to record the important addition to our knowledge of this subject effected by the zeal and attention, as well of the resident practitioners in the various infected districts, as of the numerous visitors thereto attracted.

The great and important addition to our knowledge in this respect, relates to the recognition of the preliminary diarrhœa with which at least seven eighths of the cases in most of the districts have set in. An ample collection of facts illustrative of this point has recently been published by the Central Board of Health, in the second number of the *Cholera Gazette*. We extract, in a condensed form, the report on this subject, by Dr. Macann, an active and talented officer, at present superintending the districts of Northumberland and Durham.

"With reference to the questions which have been lately addressed to me from the Central Board, on the subject of diarrhœa as connected with cholera, I have now to state, that my observations and inquiries, in this part of the country, Sunderland, Newcastle, and Gateshead, all concur in leading me to adopt the following conclusions on that subject.

"1st. That the appearance or invasion of the proper choleric symptoms, viz., the vomiting and purging of fluids, neither feculent nor bilious, with cramps and prostration, was preceded, in a great majority of the cases which have hitherto occurred, by a marked relaxation of the bowels, that is, by frequent fluid dejections, constituting the complaint denominated simple diarrhœa.

"2nd. That this diarrhœa, in a great majority of cases, presented for a time no peculiar character, so that no man could tell, when called early to a patient labouring under it, whether an attack of cholera was impending or not.

"3rd. That in many cases, however, the dejections were, from an early period, more analogous to those which take place in cholera than in diarrhœa, that is, were more fluid, whiter and less feculent than they usually are in the latter complaint; and that in many of the other cases they assumed more or less of that character before the invasion or appearance of the proper choleric symptoms already referred to.

"4th. That cases of disease (as in paragraph 2) have been met with in all classes of society, and been successfully treated as simple diarrhœa, by the ordinary remedies, viz., mild laxatives, astringents, opium, &c.

"5th. From the preceding observations it would appear, that in this country, hitherto, an attack of cholera has, generally speaking, been preceded by a relaxed state of the bowels; and that the dejections connected therewith have, sooner or later, presented to the careful observer, some indications of the danger to which the patient was exposed.

"6th. For this reason, an early and assiduous attention to bowel complaints of all kinds, even the most trifling, must, as a measure of precaution and safety, be of the highest importance to all persons residing in or near infected districts."

With respect to the other symptoms or stages of the malady, little has been added to the amount of the information already laid before our readers. In elucidation of the blue or collapse stage, we have ap-

pended to this article a copy taken from a drawing of a girl, named Henry, aged 14, who died in Sunderland, after a few hours illness, and whose case was described in a letter to the Editor of this journal (p. 402). The plate conveys a clear and unexaggerated idea of the peculiar leaden hue of the face, hands, and feet; the extraordinary shrinking of the features, the collapse of the eyes, and sudden metamorphosis of the plump features of youth into the shrivelled physiognomy of old age and disease, which the cholera not unfrequently effects in the lapse of a few swift hours. The plate we now present our readers is the first which has been published in this country. M. Dalmas, one of the French commissioners to Poland, has also laid before the *Academie de Médecine* of Paris a series of plates illustrative of the peculiarities of physiognomy in the various stages of cholera. We believe, however, that that delineated in our drawing is the only stage which requires illustration.

Before we proceed beyond the symptoms of the disease, it is necessary to allude cursorily to a point of some interest—namely, the question as to the identity of the present epidemic with the cholera which commonly prevails in this country. In our first article, from a rigid analysis of the symptoms of both diseases, we concluded that they were essentially different; for while, in the disease to which we were long accustomed, vomiting and purging were invariably present—and while symptoms of irritation generally prevailed in the onset of the attack—while preliminary diarrhœa was seldom if ever noticed, and consecutive fever never witnessed in the one but as an accidental complication; in *the* cholera of recent introduction, vomiting and purging are frequently absent during the most marked stage, no symptoms of irritation are perceptible, preliminary diarrhœa is almost of constant occurrence, and consecutive fever occurs as a part of the disorder. Respecting this subject some ingenious observations have just issued from the press from the pen of Mr. Thackrah of Leeds,* who witnessed a serious epidemic of *English* cholera in 1825, and who, from the cases he then observed, inclines to the belief in the identity of the diseases. We regret, however, to be still compelled to differ from so able and accurate an observer; but as we have not space at present to lay the part of his pamphlet before our readers in which he presses his views on this question, we are in justice and candour bound to recommend it to the general perusal of the profession, for it contains the best account we have ever met with of the cholera which was epidemic in this country in 1825, and is consequently valuable as affording us the means of fairly contrasting the two diseases.

But few opportunities have been afforded in this country for the improvement of our knowledge of the anatomical pathology of the affection. On the whole, we believe that not more than a dozen of dissections have taken place, and in these, no appearances worthy of note have been detected. The pathology of the malady has, however, been investigated by another and perhaps a more conclusive method; numerous analyses of the blood and dejected matters having been performed, in Newcastle, by Dr. O'Shaughnessy, and of the blood, in Sunderland, by Dr. Clanny. The results obtained by each experimentalist coincide in denoting an immense loss of water and deficiency of saline matter in the blood, while Dr. O'Shaughnessy has detected the materials which are absent in the blood, in the peculiar dejected fluids. This gentleman has, we understand, laid a voluminous report, describing his experiments, before the Central Board of Health, and until its publication, we, of course, do not feel ourselves competent to allude minutely to the probable inferences to which his analyses may lead. Dr. Clanny having, however, published his mode of analysis of the blood, and having presented us with a physiological explanation of the disease founded on its results, we are bound to make both the process and the explanation, the subject of a few passing remarks.

Divesting Dr. Clanny's statements of extraneous details, we find that in the statement of the results of his analysis of the blood, FREE *carbon* is enumerated as one of its ingredients, and even the quantity thereof minutely specified; and to the prevalence of this FREE *carbon* in the blood of cholera patients, most, if not all, of the

* Cholera, its Character and Treatment, &c. By C. T. Thackrah, Leeds. 8vo. pp. 60. Lond. Longman and Co. 1832.

symptoms of collapse and asphyxia are attributed.

Now, our duty to our readers obliges us to state, that such an ingredient as *free carbon* has never yet been detected in blood by any of the great chemists who have hitherto made that fluid the subject of analysis. Physiologists, mere theoretical physiologists, have ever and anon, it is true, fondly speculated on its presence, and thence derived many hypotheses of great ingenuity. Again; according to the *mode of analysis* published by Dr. CLANNY, no such ingredient is sought for, or even alluded to. We consequently cannot implicitly admit either the experimental results or the pathological inferences of this gentleman, until this source of obscurity is satisfactorily explained. We should also add, in further justification of our doubts as to the accuracy of Dr. Clanny's results, that owing to *coagulation* of the serum being alone recommended by him, and no directions being given for its *evaporation to actual dryness*, a considerably greater excess of solid matters, and deficiency of water, is represented, than actually can exist.

In addition to the results already mentioned, Dr. O'SHAUGHNESSY further assures us, that his experiments fully disprove the extravagant assertions of M. HERMAN of Moscow, and that he has obtained urea from one of the specimens of cholera blood on which he operated.

As far as the preceding statements entitle us to reason, it is obvious that we must refer many of the symptoms of the malady to mechanical hyperœmia, or an inspissation of the blood, thus rendered too thick for pulmonary circulation. That the leading symptoms may thus receive a satisfactory explanation, cannot be questioned; and we therefore wait with anxiety the publication in detail of the recent inquiries; and their further prosecution by those professional gentlemen now resident in the theatre of the epidemic, to whom the department of the chemical pathology of cholera has been confided by the Edinburgh Board of Health.

TREATMENT.

The treatment of the disease next demands notice; and here also it gives us much satisfaction to note the degree of improvement which has taken place in this department. By the adjustment of suitable therapeutic measures to the preliminary diarrhœa, hundreds of lives, we are satisfied, have already been saved; and by the future treatment of this stage we entertain sanguine hopes that the future amount of mortality will be materially diminished. It is unnecessary for us to dwell particularly on this topic. We have before stated, that the cholera diarrhœa was, while a preliminary symptom, equally amenable to ordinary remedial measures with that diarrhœa, the proper treatment of which is familiar to every practitioner.

In the management of the second, or collapse stage, some improvement has also been effected, by the introduction of the use of mustard emetics, a practice first recommended by Dr. Gibson, at Sunderland, and put into operation by Dr. Lindsey. The immediate restoration of the pulse has, in many cases, followed the action of the remedy, and the patient has passed through an apparently hopeless cold stage. The mustard has doubtless, however, failed in many cases, but in these it is questionable whether any mode of medication could have been successful; at any rate, the medicine promises to be of great utility, when administered at a sufficiently early period. Accordingly we find that the Central Board of Health has officially recommended the employment of this emetic, in doses of three teaspoonsful of mustard to half a pint of warm-water. We should add, however, that in children and other very young persons, and in individuals so paralysed by the disease that their powers of deglutition are impaired, some caution should be observed in its administration. We offer this suggestion in consequence of having been informed, that in more than one case such severe spasms of the glottis, and irritation of the muscles of the larynx, were induced by the acrid powder, that the patients nearly perished of instant suffocation. For the same reason, the mustard should be very cautiously mixed with its aqueous vehicle; for when hastily done, lumps are apt to form in the mixture, containing little parcels of *dry* powders, which explode, like the pollen of a plant, when pressed by the muscles engaged in the act of deglutition.

The inhalation of oxygen gas, and the injection of the same into the rectum, were

tried at Sunderland; but the practice was attended with no beneficial result. In Newcastle Mr. Baird is stated to have used a tobacco injection, in six cases, with great advantage; but in another case treated thus by Dr. Lawrie, almost instant death supervened on the experiment. This circumstance should evidently teach other practitioners the necessity of observing the utmost caution in the institution of these peculiar clinical researches.

In one severe case at Haddington, the galvanic battery has been resorted to, and it is stated with the utmost success. We are unwilling to check the feelings of pleasure and security these solitary experiments usually create in the public mind, but we still cannot admit, without further evidence, that the success in the Haddington case was the effect of the remedy employed.

The next practical improvement in the treatment of this disease seems to us of considerable importance, especially when we view it in connexion with the recent chemical researches which we have already noticed. We allude to the employment of copious injections of diluent fluids into the rectum,—a measure first practised by Dr. Gibson in one of the cholera hospitals in Newcastle, and since repeated with the most encouraging results. A communication from Dr. Clanny, inserted in this day's Lancet, bears upon this subject. It may be interesting perhaps to state, that Sydenham recommended the same process in the treatment of the violent cholera he so ably describes.

It is foreign to our design in the construction of this supplementary article, to enter minutely into the details of *all* the methods of treatment yet employed in this country. Having noticed the most remarkable, we must refer our readers to that part of the first paper which relates to the treatment, and in which will be found the observations which would now apply to the other modes of practice adopted in Great Britain up to the period at which we write.

SANITARY MEASURES.—BENEFIT OF COAST QUARANTINE.—CONCLUSION.

The government of the country having wisely profited by the experience of other nations, and declined to gratify the charitable and economical desire, expressed so loudly in certain quarters for the institution of an inland system of cordons, quarantines, and lazarettoes, we are happily enabled to limit our remarks on the subject of sanitary measures to the effects of the coast quarantine, and the nature of the local measures adopted in the infected districts.

Concerning the first, we have no hesitation in recording our opinion, that to the measures of quarantine observed for some time past upon our coasts with reference to the vessels from the rivers Wear and Tyne, vexatious as it may have been to individual and local interests, is the metropolis still indebted for its exemption from the disease; and the sum total of our commercial relations beholden for the comparative freedom they still maintain in the ports of other nations. We are aware that, in advancing these opinions, we expose ourselves to misconception and obloquy, but we confidently turn to our remarks regarding the numbers of seizures and deaths on board of vessels in the various quarantine stations, as one of our chief reasons for maintaining the first; while, in the second, we are supported by the simple fact, that had not the Sunderland, Newcastle, and Shields vessels been placed in rigid quarantine, every port in Great Britain would forthwith have been so treated by the other powers with which we hold commercial intercourse. On this point we need scarcely offer any further comment. An inconsiderate morning journal has, day after day, eked out its meagre columns with sneers at the executive for permitting inland, while they prohibited maritime intercourse. Had the writer in question known aught of the sanitary history of Europe during the preceding summer, he would have been satisfied that a military cordon of 50,000 men, as disciplined as the best troops of Prussia, or of Great Britain, could no more effectually have restrained inland intercourse in this country, than Mrs. Partington could have kept out the tide by the employment of her mop. But why not then give up maritime restriction also? First, we reply, in accordance with the general interest of the country as connected with the regulations of foreign powers; and, secondly, on the same principle which—if we expected a visit from midnight marauders in a house having

two windows, one of which could not be secured—would induce us to barricade that which admitted of fortification, and trust to our personal vigilance for the protection of the other.

Of the measures of local medical police which have been practised in the infected districts, a brief notice will prove sufficient. In Sunderland a cholera hospital was established, consisting of one large room, subdivided by canvas curtains, and furnished with nurses and medicines, but destitute of that most essential requisite, a resident medical attendant. Of this hospital it is also necessary to remark, one great and most important oversight committed in its arrangement, and one which also existed in Newcastle, viz.: that of the wards being *up two flights of stairs*, so that the patient was obliged to be carried up in a *vertical* position, a proceeding on the danger of which we need not offer any remark. We trust this notice will induce future providers of hospitals to construct the wards *on the ground floor alone*.

In all the infected districts, the dead have, up to this time, been interred within twelve hours from their decease. This practice has been productive of much bitter feeling and animosity towards the parties engaged in carrying it into execution. On the whole, however, we think the practice desirable; but we would suggest that the burials should never be performed until at least *ten* hours have elapsed. The circulation of similar pieces of scandal to that which recently figured in the Scottish papers will thus be effectually prevented.

A most important measure of local medical police, recommended by the Central Board, relates to the suppression of vagrancy, the provision for beggars, and the disinfection of wearing apparel by heating it to 212°; the Board having very properly abandoned the usual ridiculous practice of chlorine ablutions.

But by far the most important step, and the most likely to induce salutary effects, is that to which we have already alluded in connexion with the Edinburgh disease; we mean the removal of the families of persons deceased of cholera to the Queensberry hospital. We have no doubt that this practice will materially retard the extension of the malady in Edinburgh, and if sagaciously and judiciously imitated in other places, will effect an equally fortunate result.

The last measure of sanitary police to which we shall allude is the recommendation just issued by the Central Board of Health, relative to the organisation of cholera hospitals in London. The Board recommends:—

"That each parish and district of this capital and suburbs should, without delay, provide—One or more houses to be used in case of necessity, as temporary cholera hospitals; six beds complete in each; a resident nurse and assistant; a mode of conveyance for cholera patients, in which they may be carried in a *horizontal posture*; and make such arrangements as to be able to command the services of a medical gentleman in each hospital so soon as required."

The prudence of this advice requires no remark; we trust it will be at once adopted not only throughout the metropolis, but in every large town in extensive communication with the districts now infected.

It now only remains for us briefly to allude to the productions which have issued from the press of this country since the publication of our first narrative. Those immediately connected with the British epidemic we have already adverted to. Of the others, Mr. Orton's treatise on cholera deserves especial notice and approbation. It is, in truth, a masterly production, and does the highest credit to the author's industry, ability, and research. Many other excellent volumes stand on our list, besides numbers of pamphlets, small and large, and heaps of worthless productions, the full titles of the whole of which would alone occupy more space than this article.

We thus conclude our *coup d'œil* of the progress of the malady in Great Britain since its first introduction. We have omitted the notice of many circumstances on which we could not enter at sufficient length consistently with our space, and which did not admit of being considered in a cursory manner; and we now take leave of the subject, not to return to it at such length until an equal period of time shall have again elapsed. By the expiration of that interval we may predict, that should the disease unhappily have spread in the

metropolis of the North, our knowledge of all the obscure points in its history will, we hope, be as extensively improved as human zeal and professional talent can accomplish. The arrangements made in Edinburgh for the scientific investigation of the disease, exceed in perfection anything of the kind hitherto attempted. We heartily wish the eminent individuals therein engaged, success in their researches, and immunity from the effects of the dangers with which they may be surrounded.

RELIGIOUS WARNING TO THE MEMBERS OF THE LONDON MEDICAL SOCIETY.

A curious case of otitis in a child, in which the whole of the labyrinth of the internal ear was thrown off, was related at the last meeting of this Society, but we have not room for a report of it in our present Number.

At the close of this meeting the following singular proof of the contagious character of the Rev. Mr. Irving's newly-broached doctrines was exhibited. It appears that amongst other converts to the belief in modern prophecy, the power of working miracles, and an approaching manifestation of the Spirit of God on earth, is Mr. Amesbury, the surgeon, and generally-known author of a work "On Fractures." This gentleman is a member of the Society, and was present on the above occasion. One of the duties enjoined by the leaders of the "New Church" on its disciples, is that of warning others "to flee from the wrath that is to come," and of assuring the world, in the words of the "prophets" at the Scotch Church, that "He will descend," and "thoroughly purge the floor of his garner." Accordingly, whenever an opportunity occurs of cautioning individuals and families, without using objectionable forwardness, the disciples of Mr. Irving do so, as the commissioners of the Supreme Being. It appears that this proceeding is not limited to strictly private occasions, for on Monday evening, on the verge of ten o'clock, Mr. Amesbury, labouring we doubt not under the firmest conviction of the propriety and imperative nature of his mission, arose and sedately addressed the President to the following effect:—

Mr. Amesbury.—I have an important message, Sir, to deliver to this Society, and beg leave to be permitted to do so.

The President, who appeared to be aware, from previous report probably, of the nature of the proffered communication, said,—However important its nature, Sir, I submit to you whether it would not be more befitting to make the communication on another occasion, or, perhaps, when the members have left the room. [It is sometimes usual for them to assemble after the meeting in an apartment below, affording sufficient opportunity for the accomplishment of Mr. Amesbury's purpose.]

Mr. Amesbury acquiesced, and the members present, their curiosity being somewhat excited, immediately afterwards retired to the room in question, where Mr. Amesbury declared himself to be commissioned to deliver "a message of grace and mercy;" and, after offering up a short prayer for the opening of the hearts of his hearers, assured them that a great religious change was at hand, and that "the gifts of the Spirit were about to be renewed with signs and wonders," and he advised them to take counsel before it was too late. We cannot charge ourselves to state more precisely the tenor of the speaker's communication, but this was the spirit of it. On being asked what proof he could afford them of the authenticity of his mission, Mr. Amesbury merely said, we believe, that he was *assured* of the truth and divine origin of all he had made known. The proceeding was gone through with gravity, but we are not aware that a conviction of the alleged "truths" was brought home to the minds of any one present.

Since writing the above, we have heard that the disciples of Mr. Irving are circulating the following statement as an additional confirmation of their averment that the individuals who declaim in the "unknown tongue," at the Scotch Church, are moved thereto by divine influence. It is said by them "that a medical man has declared the human thorax to be quite unequal to the efforts made by the speakers at the church, unless it were expressly strengthened by miraculous agency, for that without such intervention *it would on those occasions be instantly destroyed or deprived of life.*" We understand that the medical gentleman on whose authority this "testimony" is now being generally quoted, is Mr. Amesbury, who suffers no opportunity of social intercourse to pass without admonishing those in whose company he may be, after the fashion of his message to the members of the above society.

CORRESPONDENTS, &c.

Errata in our present Number.—In Dr. Burnes's case, p. 662, col. 2, line 27, for "blade," read *base*; p. 663, in the engraving of the fork, the letter B should be one inch from the point A.

Erratum.—In Mr. Coley's paper, No. 435, col. 2, line 7 from the bottom, for "os malæ," read *os sex illare superius*.